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EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

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ERRATA IN THE NUMBER OF THE JOURNAL FOR OCTOBER 1857.

Page 257, line 18, *for* Cephalaspis (Pteraspis) ornatus, Egerton; *read* Cephalaspis ornatus, Egerton;

Mr Symonds also requests us to state that the Cervine horn from the Severn Drift (see page 328) is not that of Megaceros, but probably of Cervus Bucklandi, Owen.

Page 337, line 29, *for* Mejacapsæa Polyandria, *read* Megacarpæa polyandra,
— line 31, *for* crucifera *read* Cruciferæ
— line 33, *for* Brickman *read* Buckman
Page 361, line 12, *for* United States *read* H. M. Ship
— line 19, *for* Bavaria *read* Banana
Page 362, lines 16, 20, and 23, *for* Schrietzlein *read* Schnitzlein
— line 26, *for* Maudin *read* Naudin,
— line 51, *for* Juice *read* Source—and *for* Decaisine *read* Decaisne

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THE

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On the Supposed Prevalence of One Cranial Type throughout the American Aborigines. By DANIEL WILSON, LL.D., Professor of History and English Literature, University College, Toronto.*

Among the various grounds on which Columbus founded his belief in the existence of an undiscovered continent beyond the Atlantic, especial importance was attached to the fact that the bodies of two dead men had been cast ashore on the island of Flores, differing essentially in features and physical characteristics from any known race. When at length the great discoverer of this western world had set his foot on the islands first visited by him, the peculiarities which marked the gentle and friendly race of Guanahanè were noted with curious minuteness; and their "tawny or copper hue," their straight, coarse, black hair, strange features, and well-developed forms, were all recorded as objects of interest by the Spaniards. On their return the little caravel of Columbus was freighted not only with gold and other coveted products of the New World, but with nine of its natives, brought from the Islands of San Salvador and Hispaniola,—eight of whom survived to gaze on the strange civilization of ancient Spain, and to be themselves objects of scarcely less astonishment than if they had come from another planet. Six of these representatives of the western continent, who accompanied

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* Read before the British Association for the Advancement of Science, at Dublin, Sept. 7, 1857.

Columbus to Barcelona, where the Spanish Court then was, were baptized with the utmost state and ceremony, as the first fruits offered to Heaven from the new-found world. Ferdinand and the enthusiastic and susceptible Isabella, with the Prince Juan, stood sponsors for them at the font; and when, soon after, one of them, who had been retained in the Prince's household, died, no doubt as to their common humanity marred the pious belief that he was the first of his nation to enter heaven.

Such was the earliest knowledge acquired by the Old World of the singular type of humanity generically designated as the Red Indian; and the attention which its peculiarities excited when thus displayed in their fresh novelty has not yet exhausted itself, after an interval of upwards of three centuries and a half. That certain special characteristics in complexion, hair, form, and features, do pertain to the whole races of this continent is not to be disputed; and these prevalent characteristics were so generally noted, to the exclusion of all others, that Ulloa, and after him others of the Spanish explorers of the New World, remarked: *He who has seen one tribe of Indians has seen all.* In the sense in which this remark was first made, and by Spaniards who knew only of Central America and the tropical region of the Southern continent, there was nothing in it to challenge. But that which was originally the mere rude generalization of a traveller has been adopted in our own day as a dogma of science; and the universality of homogeneous characteristics of the aboriginal tribes and nations of America, with the exception of the Esquimaux, is assumed as an established postulate for the strictest purposes of scientific induction, and has been repeatedly affirmed in those very words.

Such authorities as Robertson the historian, and Malte Brun, may be classed along with the first Spanish observers, in the value to be attached to their sweeping generalizations. "The Esquimaux," says the former, "are manifestly a race of men distinct from all the nations of the American continent, in language, in disposition, and in habits of life. But among all the other inhabitants of America there is such a striking similitude in the form of their bodies, and the qualities of their minds, that, notwithstanding the diversities occasioned by the influence of climate, or unequal progress of improvement, we must pronounce them to be descended from one source."* Malte Brun, with more caution, simply affirms, as the result of a long course of physiological observations, "that the Americans, whatever their origin may be, constitute at the present day a race essentially different from the rest of mankind."† But greater importance is to be attached to the precisely-defined views of Humboldt, in so far as these are not—like those of so many other writers on this subject—a mere reproduction of the opinions of Morton. Humboldt remarks in the preface to his *Researches:* "The nations of America, except those which border the Polar circle, form a single race, *characterized by the formation of the skull, the colour of the skin, the extreme thinness of the beard, and the straight glossy hair.*"

Very few and partial exceptions can be quoted to the general unanimity of American writers,—some of them justly regarded as authorities in ethnology,—in reference to this view of the nations of the whole American continent, north and south. With the solitary exception of the Esquimaux, they are affirmed to constitute one nearly homogeneous race, varying within very narrow limits from the prevailing type, and agreeing in so many essentially distinctive features, as to prove them a well-defined variety, if not a distinct species of the genus *Homo.* Prichard, Lawrence, Wiseman, Knox, Squier, Gliddon, Nott, and Meigs, might each be quoted in confirmation of this opinion, and especially of the prevailing uniformity of certain strongly-marked cranial characteristics: but the fountain-head of all such opinions and views is the justly-distinguished author of the *Crania Americana,* Dr Morton of Philadelphia. His views underwent considerable modification on some points relating to the singular cranial confirmation observable in certain skulls found in ancient American graves; especially in reference to the influence of artificial means in perpetuating changes of form essentially different from the normal type; but the tendencies of his matured opinions all went to confirm his original idea of universal approximation to one cranial type throughout the New World. In some of his latest recorded views he remarks, as the result of his examination of a greatly extended series of Peruvian crania, — "I at first found it difficult to conceive that the original rounded skull of the Indian could be changed into this fantastic form; and was led to suppose that the latter was an artificial elongation of a head remarkable for its length and narrowness. I even supposed that the long-headed Peruvians were a more ancient people than the Inca tribes, and distinguished from them by their cranial configuration. In this opinion I was mistaken. Abundant means of observation and comparison have since convinced me that *all these variously-formed heads were originally of the same rounded shape.*"

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* Robertson's America, B. IV. In relation to languages, this difference between the Esquimaux and the Indians is no longer maintained.

† Malte Brun, Geog. Lib. xxv.

Such are the latest views of Dr Morton, as set forth in the posthumous paper on *The Physical Type of the American Indians,* contributed by him to the second volume of Dr Schoolcraft's "History of the Indian Tribes," and edited for that work by his friend and fellow-labourer, John S. Phillips. In that same final contribution to his favourite science, Dr Morton's matured views on the cranial type of the American continent,—based on the additional evidence accumulated by him, in the interval of twelve years which elapsed between the publication of the *Crania Americana* and the death of its author,—are thus defined: "The Indian skull is of a decidedly rounded form. The occipital portion is flattened in the upward direction, and the transverse diameter, as measured between the parietal bones, is remarkably wide, and *often exceeds the longitudinal line.** The forehead is low and receding, and rarely arched as in the other races; a feature that is regarded by Humboldt, Lund, and other naturalists as a characteristic of the American race, and serving to distinguish it from the Mongolian. The cheek-bones are high, but not much expanded; the maxillary region is salient and ponderous, with teeth of a corresponding size, and singularly free from decay. The orbits are large and squared, the nasal orifice wide, and the bones that protect it arched and expanded. The lower jaw is massive and wide between the condyles; but, notwithstanding the prominent position of the face, the teeth are for the most part vertical."* The views thus set forth by him who has been justly designated "the founder of the American school of Ethnology,"† have been maintained and strengthened by his successors; and scarcely any point in relation to Ethnographic types is more generally accepted as a recognised postulate than the approximative homogeneous cranial characteristics of the whole American race. A distinction, indeed, is made by Morton, and to some extent recognised by his successors, between the *barbarous*, or *American*, and the *civilized*, or *Toltecan* tribes of the continent; but the distinction, according to their own view, is arbitrary, and appears alike indefinite and unsatisfactory; unless an essential difference of race, corresponding to that which is held to separate the Esquimaux from the true *Autocthones* of America, is acknowledged to exist, whereas this is expressly denied. One of the three propositions with which Dr Morton sums up the results borne out by the evidence advanced in his *Crania Americana*, is, "That the American nations, excepting the polar tribes, are of one race and one species, but of two great families, which resemble each other in physical, but differ in intellectual character."‡ Any further difficulty, arising from physical differences, is sought to be overcome by the application of the hypothesis, that "these races originated in *nations*, and not in a single pair; thus forming proximate but not identical species."§ But the difficulty is not fairly grappled with by any of the writers of "the American school of Ethnology." The closest approximation to a recognition of the legitimate deduction from such contrasting cranial characteristics is made by Dr Morton himself, where he remarks, in reference to the larger cerebral capacity of the Indian in his savage state, than of the semi-civilized Peruvian or ancient Mexican—"Something may be attributed to a primitive difference of stock; but more, perhaps, to the contrasted activity of the two races." It is to be noted, moreover, that Dr Morton distinctly recognises certain unmistakeable diversities of form into which the assumed American cranial type is subdivided. He thus remarks, in his *Crania Americana*, under the head,—*General observations on the barbarous nations composing the American Family*,—"After examining a great number of skulls, I find that the nations east of the Alleghany Mountains, together with the cognate tribes, have the head more elongated than any other Americans. This remark applies especially to the great Lenapé stock,—the Iroquois and the Cherokees. To the west of the Mississippi we again meet with the elongated head in the Mandans, Ricaras, Assinaboins, and some other tribes." But to this, Dr Morton superadds the further remark: "Yet even in these instances the *characteristic truncature of the occiput* is more or less obvious; while many nations east of the Rocky Mountains have the rounded head so characteristic of the race, as the Osages, Ottoes, Missouris, Dacotas, and numerous others. The same conformation is common in Florida; but some of these nations are evidently of the Toltecan family, as both their characteristics and traditions testify. The head of the Charibs, as well of the Antilles as of Terra Firma, are also naturally rounded; and we trace this character, as far as we have had opportunity for examination, through the nations east of the Andes, the Patagonians, and the tribes of Chili. In fact, the flatness of the occipital portion of the cranium will probably be found to characterize a greater or less number of individuals in every existing tribe from Terra del Fuego to the Canadas. If their skulls be viewed from behind, we observe the occipital outline to be moderately curved outward, wide at the occipital protuberances, and full from those points to the opening of the ear. From the parietal protuberances there is a slightly curved slope to the vertex, producing a conical, or rather a wedge-shaped outline." These opinions are still more strongly advanced in Dr Morton's most matured views, where he ascribes the same characteristics to the

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* In this statement Dr Morton would seem to have had in view his theoretical type, rather than the results of his own careful observations, unless he accepted as evidence the artificially abbreviated and flattened skulls, and even of these his *Crania Americana* furnishes only one exceptional example, from a mound on the Alabama River (Pl. LIV). "It is flattened on the occiput and os frontis in such a manner as to give the whole head a sugar-loaf or conical form, *whence also its great lateral diameter and its narrowness from back to front.*"

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* Physical Type of the American Indians. Schoolcraft's Hist., &c., ii. p. 316.

† Types of Mankind, p. 87.

‡ Crania Americana, p. 260.

§ Types of Mankind, p. 276.

Fuegian, the Indian, the tribes to the west of the Rocky Mountains, and those which skirt the Esquimaux on the north. "All possess alike the long, lank, black hair, the brown or cinnamon-coloured skin, the heavy brow, the dull and sleepy eye, the full and compressed lips, and the salient but dilated nose. The same conformity of organization is not less obvious in the osteological structure of these people, as seen in the square or rounded head, the flattened or vertical occiput, the large quadrangular orbits, and the low receding forehead;" and he goes on to reiterate the opinion that, in spite of any "mere exceptions to a general rule," the Indian of every variety "is an Indian still, and cannot be mistaken for a being of any other race." Still more, in the same final embodiment of his matured opinions, Dr Morton affirms the American race to be *essentially separate and peculiar*, and with no obvious links, such as he could discern, between them and the people of the Old World, but *a race distinct from all others.*

It is obvious that the tendency of Dr Morton's views, as based on the results of his extended observations, was to regard the most marked distinctions in American crania as mere variations within narrow limits, embraced by the common and peculiar type, which he recognised as characteristic of the whole continent, both north and south. In this opinion his successors have not only concurred, but they even attach less importance to the variations noted by his careful eye. Dr Nott, for example, remarks on the peculiarities of the very remarkable brachycephalic skull taken from a mound in the Scioto Valley, and figured the natural size in Messrs Squier and Davis's *Ancient Monuments of the Mississippi Valley:** "Identical characters pervade all the American race, ancient and modern, over the whole continent. We have compared many heads of living tribes, Cherokees, Choctaws, Mexicans, &c., as well as crania from mounds of all ages, and the same general organization characterizes each one."†

One more authority may be quoted to show that the conclusions thus early adopted by Dr Morton, and maintained and confirmed by his subsequent writings, are still regarded as among the best established and most indisputable summaries deduced from the well-ascertained data of American Ethnology. Dr J. Aitken Meigs, the editor of Dr Morton's "Catalogue of Skulls," subsequent to the transference of his greatly augmented collection to the Academy of Natural Sciences of Philadelphia, remarks, in his *Cranial Characteristics of the Races of Men* : "Through *Crania Americana*, it has long been known to the scientific world that a remarkable sameness of osteological character pervades all the American tribes from Hudson's Bay to Terra del Fuego. It is equally well known that the researches of Humboldt and Gallatin have demonstrated a conformity not less remarkable in the language and artistic tendencies of these numerous and widely scattered aborigines."*

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* Smithsonian Contributions to Knowledge, vol. i. pl. 47.

† Types of Mankind, p. 291.

Such, then, is the opinion honestly arrived at by Dr Morton, as the result of extensive study and observation, accepted and confirmed by his successors, and now made the starting-point from whence to advance to still more comprehensive and far-reaching conclusions. It is not necessary, therefore, to prove the universal recognition of this well-known ethnological postulate, by farther references to recent authorities; but there is one author, at once so distinguished among American men of science, and so peculiar from the point of view from which he has regarded the entire question of American Ethnology, as to merit special attention,—Professor Agassiz, in his *Sketch of the Natural Provinces of the Animal World, and their relation to the different Types of Man*, re-affirms the homogeneous characteristics and ethnic insulation of the American Indian on entirely novel and independent grounds. After defining the evidence on which the general opinion is based, that *the boundaries within which the different natural combinations of animals are circumscribed on the surface of the earth coincide with the natural range of distinct types of man*, he proceeds to show that America, including both its northern and southern continent, differs essentially from Europe and Asia, or Africa, in being characterized throughout by a much greater uniformity in all its natural productions, than anything which comparison enables us to trace in the Old World. He then adds: "With these facts before us, we may expect that there should be no great diversity among the tribes of man inhabiting this continent; and indeed the most extensive investigation of their peculiarities has led Dr Morton to consider them as constituting but a single race, from the confines of the Esquimaux down to the southernmost extremity of the continent. But, at the same time, it should be remembered that, in accordance with the zoological character of the whole realm, this race is divided into an infinite number of small tribes, presenting more or less difference one from another."

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* Indigenous Races of Men, p. 332.

The latest views of Agassiz, as set forth in his contribution to the *Indigenous Races of the Earth,* present us with the same opinions, advanced with additional confirmation from other data. Passing from the general zoological analogies in the distribution of species, to the special one of the monkey, he remarks on the diversity of opinions among men of science as to the genus *Cebus*, which some zoologists recognise as one species, others separate into two or three, while others again subdivide it into as many as ten:—"Here we have, with reference to one genus of monkeys, the same diversity of opinion as exists among naturalists respecting the races of man. But in this case the question assumes a peculiar interest, from the circumstance that the genus *Cebus* is exclusively American; for that discloses the same indefinite limitation between its species which we observe also among the tribes of Indians, or the same tendency to splitting into minor groups, running really one into the other, notwithstanding some few marked differences,—in the same manner as Morton has shown that all the Indians constitute but one race, from one end of the continent to the other. . . . . . In the Old World, notwithstanding the re-occurrence of similar phenomena, the range of variation of species seems less extensive, and the range of their geographical distribution more limited. In accordance with this general character of the animal kingdom, we find likewise that, among men, with the exception of the Arctic Esquimaux, there is only one single race of men extending over the whole range of North and South

America, but dividing into innumerable tribes; whilst, in the Old World, there are a great many well-defined and easily distinguished races, which are circumscribed within comparatively much narrower boundaries." To this may be added Mr Gliddon's summary of the views advanced by him, in carrying out the suggestive idea of Agassiz, in the *Monogenists and Polygenists* of the former :*—"We may now reconsider some of the practical issues of this inquiry. It has been shown, 1st, That in America, humatile men and humatile monkeys occupy the same palæontological zones. 2d, That whilst all such remains of man are exclusively of the American Indian type, the monkeys called *Hapale, Cebus, Callithrix*, &c., are equally 'terræ geniti' of this continent. . . . . Finally, that *permanence of type*, as well for humanity as for simiadæ, is firmly established in both genera, from the hour in which we are living back to a vastly remote, if not incalculable, era of unrecorded time."

Such being some of the very important and comprehensive deductions now based on the premises originally advanced by Dr Morton, it becomes of some interest to the Ethnologist to ascertain if these premises are so surely established as to be beyond all question. That some of the assumed evidence of this all-pervading conformity has been adopted on insufficient data, is manifest from the premature generalizations in relation to the holophrastic or polysynthetic character affirmed to pertain to all the languages and dialects of America, and assumed to supply the place of that grammatical unity of structure in the Indo-European languages, the establishment of which has led to such important results.

The dialects of the numerous families of American tongues multiply with the labours of their investigators. Duponceau, writing in 1822, numbered them as one thousand two hundred and fourteen. Scarcely any trace of the roots of a common vocabulary helps in the comparison of many of these diverse languages of the New World. Of some of the indigenous tongues even now spoken around the Rios and Colorado, and in more southern latitudes, the holophrastic attribute is rather assumed than known; and in more than one group, of which the Carib is an illustration, languages are found in nearly all the lowest stages of undeveloped simplicity. Nevertheless, this holophrastic or polysynthetical mode of condensing a group of words into one abbreviated term, susceptible of further modification; and of inflexion, is well worthy of the interest it has excited. This distinguishing trait, or "plan of thought of the American languages," as Dr Lieber has designated it, has yet to be applied as a philological test to many untried tongues and dialects of the New Continent; but meanwhile, some of the most comprehensive generalizations based on it seem to have been advanced in the inverse ratio of the linguistic knowledge of their advocates. Those most fitted to pronounce on the subject—as Duponceau, in his later writings, and Gallatin—most cautiously avoid general conclusions, such as the former was tempted to by earlier and less complete observations; and, as in many other inquiries, extended knowledge tends at present to complicate the question, instead of confirming the seductive theory of Duponceau, of a common philological character pervading the languages of America from Greenland to Cape Horn.

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* Indigenous Races of Men, p. 522.

The extreme interest which attaches to the investigation of the distinguishing traits already recognised as pertaining to the languages of the New World cannot be over-estimated, though it is not improbable that an exaggerated value has been assigned to the significance of their specialties. In more than one trait characteristics are recognised common both to Polynesian and African idioms; and further consideration suggests the probability that the special synthetic tendency pertains fully as much to an immature stage of development of those languages as to any specific individualizing feature born of the New World's insulation. As, moreover, the opinion advanced by Gallatin, after mature investigation, of the correspondence of the Esquimaux language to those of the true Indians of America, in the same degree that these possess elements in common, is acknowledged to be correct; the assumed philological unity of the American Indians amounts to no more than a predominance of certain linguistic tendencies analogous to such as, in the Old World, embrace a widely varied ethnic and geographic area. "Physically," says Latham, "the Eskimaux is a Mongol and Asiatic; philologically he is American, at least in respect to the principles upon which his speech is constructed."*

The same manifestation of a predisposed tendency to shape the evidence to a foregone conclusion, or to assume as special whatever varies from the normal type, may be traced in various other lines of argument; such as, for example, where, in proof of the essential ethnic difference between the Esquimaux and the true Indian of America, the traveller Herne is quoted as stating that "The Indian tribes who are their proximate neighbours on the south once excused an unprovoked massacre of Esquimaux men, women, and children, by asserting that they were a people of a different nature and origin from themselves." Such a line of argument would prove other tribes, besides the Esquimaux, to be of a different nature and origin. Similar evidence, indeed, might suffice to show that the Anglo-Saxons of the ancient kingdom of Northumbria, so soon as they were separated by the political boundary line of the Sark or Tweed, became essentially different races; for assuredly no Indians and Esquimaux could manifest more deadly hatred to each other than that which intensifies the wild vigour of the old Border Minstrelsy.

But it is not necessary to go beyond the American pale for similar evidences. The Guanches, discovered by Columbus in 1492, attracted his attention by their gentle manners and inoffensive habits, and from them he learned of the Caribs, a fierce and warlike people in the neighbouring islands and the mainland, of whom they lived in constant dread, and who subsequently became familiar to the Spaniards as a ferocious, crafty, and revengeful race, delighting in cannibalism.

Moreover, the great Admiral failed not to note the marked distinction between the fair complexion of the Guanches and the reddish-olive of the ferocious Caribs. Both Humboldt and Morton acknowledge the existence of considerable varieties in colour and complexion, from nearly white to a dark brown. The latter writer, indeed,—guarding against possible deductions from such an admission, adverse to his favourite theory of a universally predominating conformity in all the essential characteristics of the American aborigines,—adds: "These differences in complexion are extremely partial, forming mere exceptions to the primitive and national tint that characterizes these people from Cape Horn to the Canadas. The cause of these anomalies is not readily explained; that it is not climate is sufficiently obvious; and whether it arises from partial immigrations from other countries, remains yet to be decided."*

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* Varieties of Man, p. 290.

The stronghold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents, is the supposed uniformity of physiological, and especially of physiognomical and cranial characteristics; an ethnical postulate which has not yet, so far as I am aware, been called in question.

On first visiting the American continent, and enjoying the opportunity of judging for myself of the physical characteristics of the aboriginal race of the forests, I did so under the full conviction of meeting with such a universal approximation to the assumed normal type as would fully bear out the deductions of previous observers, and especially of one so persevering in the accumulation of the requisite materials on which to base a legitimate result, as the author of the *Crania Americana.* I visited Philadelphia with a special view to examine the valuable collection of Crania formed by Dr Morton, and looked with lively interest on some of the most striking illustrations which it affords of the typical form assigned by him to the American race. Unfortunately, at the period of my visit (September 1853), extensive alterations in progress on the buildings of the Academy deprived me of the opportunity for such detailed observations as were requisite for drawing any just comparison between these data and the comprehensive deductions founded on them by their collector. When, therefore, I proceeded more recently to open some Indian graves in Canada, and to endeavour to procure crania from others on ascertaining of their disturbance, it was solely with a view to possess myself of one or two specimens of the peculiar American type of cranium, which possessed a special interest in my eyes from its approximation to the ancient brachycephalic skull, familiar to me, as found in one important class of early British barrows. It was accordingly, simply with a sense of disappointment that I found the results of repeated efforts, in different localities, supplied me with crania which, though undoubtedly Indian, exhibited little or no trace of the rounded form, with short longitudinal diameter, so strikingly apparent in the ancient crania of Central America and the Mounds. Appreciating, as I did, the invaluable labours of Dr Morton,—which will be more fully prized as the important science they tend to elucidate commands a wider attention and more careful study, —it did not occur to me at first to question any of the results so frequently reiterated by him, and repeatedly confirmed by the concurrence of later writers. Slowly, however, the idea has forced itself upon me, that, to whatever extent the affirmed typical form of the American cranium is found to prevail in other parts of the continent, the crania most frequently met with along the north shores of the great lakes are deficient in some of its most essential elements.

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* Crania Americana, p. 70.

In order to institute such a comparison as will satisfactorily test this question, it is necessary to define the essential requisites of the American type of cranium; for neither Dr Morton nor his successors have overlooked the fact of some deviation from the supposed normal type, not only occurring occasionally, but existing as a permanent characteristic of certain tribes, including those to which I have more particularly to refer. Dr Morton recognised a more elongated head as pertaining to certain tribes, of which he names the Lenapé stock, the Iroquois, and the Cherokees, to the east of the Alleghany Mountains; and the Mandans, Ricaras, and Assinaboins, to the west. But such elongation he speaks of as a mere slight variation from the more perfect form of the normal skull; and he adds: "even in these instances the characteristic truncation of the occiput is more or less obvious."* So also Dr Nott, after defining the typical characteristics of the American cranium, remarks,—" Such are more universal in the Toltecan than the

* Crania Americana, p. 69.

barbarous tribes. Among the Iroquois, for instance, the heads were often of a somewhat elongated form, but the Cherokees and Choctaws, who, of all barbarous tribes, display greatest aptitude for civilization, present the genuine type in a remarkable degree. My birth and long residence in southern states have permitted the study of many of these living tribes, and they exhibit this conformation almost without exception. I have also scrutinized many Mexicans, besides Catawabas of South Carolina, and tribes on the Canada Lakes, and can bear witness that the living tribes everywhere confirm Morton's type."*

We cannot err in taking the very interesting cranium found by Dr Davis and Mr Squier in a mound in the Scioto Valley, Ohio, as an example of the true typical head; for it is produced as such by Dr Nott, in the "Types of Mankind," and it is described, in the words of Dr Morton, in Dr Meigs's *Catalogue of Human Crania, in the collection of the Academy of Natural Science of Philadelphia*, issued during the present year by order of the Academy, as—"an Aboriginal American; a very remarkable head. This is, perhaps, the most admirably formed head of the American race hitherto discovered. It possesses the national characteristics in perfection, as seen in the elevated vertex, flattened occiput, great interparietal diameter, ponderous bony structure, salient nose, large jaws, and broad face. It is the perfect type of Indian conformation, to which the skulls of all the tribes from Cape Horn to Canada more or less approximate." As shown by the front view of this skull it presents no trace of pyramidal conformation.

Of this skull the measurements which involve the most essential typical elements, and so furnish precise materials for comparison, are,—

Longitudinal diameter	6·5	inches.
Parietal „	6·	„
Vertical „	6·2	„
Intermastoid arch	16·	„
Horizontal circumference	19·8	„

So that, in fact, the cranium very closely corresponds in its measurements in length, breadth, and height. Still further, it may be noted, on examining the full-sized view of the skull, as given by Messrs Squier and Davis (Pl. XLVII.) that the singular longitudinal abbreviation of this skull is nearly all posteriorly. A line drawn through the meatus auditorius externus in profile, parallel to the elevated forehead, divides it into two unequal parts, of which the anterior and posterior parts are nearly in the ratio of two to one. To this type the ancient Peruvian and Mexican crania unquestionably approximate. Of one of the former, from the Temple of the Sun, (Pl. XI.) Dr Morton remarks: "A strikingly characteristic Peruvian head. As is common in this series of skulls, the parietal and longitudinal diameters are nearly the same;" viz., longitudinal diameters 6·1, parietal diameter 6. So far, therefore, as such evidence goes, it appears to justify the conclusion arrived at by Dr Morton, that the people represented by the Mound skulls in his possession "were one and the same with the American race, and probably of the Toltecan branch."*

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* Types of Mankind, p. 441.

The conformity affirmed to exist between the ancient Mexican and Peruvian skulls, and those of the modern barbarous tribes, may also be so far asserted as a partial approximation in relation to some of them, and appears to receive a fuller confirmation when carefully selected examples are referred to; as a sufficient number occur to indicate the occasional re-appearance of some of the most striking typical peculiarities. Such re-appearance of the extremest typical forms is not, however, peculiar to this continent. I possess measurements of a singular modern (female) skull in the collection of Dr John Struthers of Edinburgh, which reproduces in all its strongest features the ancient British brachy cephalic head; and I have in view more than one living illustration of the same sort:—one, for example—a gentleman of education and intelligence—with such an elevation of the vertex, flattened occiput, and short longitudinal diameter, as, judging by the eye, would more nearly approach the measurement of the Scioto Mound cranium than that of any living Indian I have seen.

Of a similar nature is the correspondence pointed out by

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* Crania Americana, p. 229.

Dr Nott* between the Scioto Mound skull and that of a Cherokee chief who died a prisoner near Mobile in 1837. In this example, in so far as can be judged from the comparison of both by drawings in profile without precise measurements, the points of agreement are indisputable, though even here amounting to no more than an approximation. The vertical occiput of the ancient skull—more markedly vertical in the original drawing than in the small copy—is only partially represented in the other; the square form of the ancient profile in the coronal region, becomes conoid in the modern one; and the intersecting line drawn through the meatus auditorius externus shows a very partial reproduction in the modern example of the remarkable preponderance of posterior cerebral development, which—if not produced by artificial means—is the most singular characteristic of the ancient head.

But while acknowledging such approximation of the selected modern Cherokee cranium to the ancient type, neither the legitimate deductions following from this, nor from the other examples referred to by Dr Nott, appear to bear out his conclusions, that not only that type "is found among tribes the most scattered, among the semi-civilized and the barbarous, among living as well as among extinct races;" but "that *no foreign race has intruded itself in their midst, even in the smallest appreciable degree.*" The examples of Cherokee heads referred to in the Table of Anatomical Measurements in the *Crania Americana,* in so far as they fairly represent the cranial characteristics of this tribe or nation, seem to indicate that the Mobile chief is an exceptional case; and this is further borne out by the special example selected by Dr Morton, and figured in his great work: "The head of a Cherokee warrior who was known in the army by the name of John Waring." The following are its most characteristic measurements, exhibiting such a wide divergence from the normal type, as illustrated in that of the Scioto Mound, as to substitute contrast for comparison:—

Longitudinal diameter	7.2
Parietal ,,	5.3

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* Types of Mankind, p. 442.

Vertical diameter	5.3
Intermastoid arch	14.1
Horizontal circumference	19.1

In the typical head the longitudinal, parietal, and vertical diameters closely correspond; in this the excess of the longitudinal over the parietal and vertical diameters is such as is rarely exceeded in the modern Anglo-Saxon, or even the longer sub-Celtic head. Yet, that such an excess in the longitudinal diameter did not present to the experienced eye of Dr Morton any striking deviation from the form of the modern Indian head is proved by his noting of this very example: "Nor is there anything remarkable in the form of the skull."

Bearing in remembrance, then, the partial nature of the approximation so far apparent between the ancient and modern American cranium, personal observation leads me to believe that such is to be found,—with exceptional instances of closer affinities, and also with important divergences from the typical Indian form and character, not exceptional, but pertaining to the whole nation,—among the still numerous examples of the Algonquin stock, as represented by the Chippeways. Of these I have examined, and compared by the eye, many at widely scattered locations: on Lake Simcoe and the Georgian Bay; at Mackinaw in Lake Huron, and at Sault St Marie; at Ontonagon, La Point, the Apostle Islands, and the St Louis River, on Lake Superior; as well as those encountered in such chance opportunities as occur in the neighbourhood of Niagara Falls, and on the streets of our Canadian towns and villages. Physiognomically they present the wide and prominent mouth, high cheek-bones, and broad face, so universally characteristic of the American Indian; but they by no means present in a remarkable degree the wide and massive lower jaw, which has been noted as of universal occurrence among the Red Indians. Still more noticeable is the absence of the aquiline nose, so characteristic generally of the true Indian in contradistinction to the Esquimaux. The eye may be fully depended on for physiognomical characteristics; it is of much less value in testing variations from any assumed cranial type, especially in reference to comparatively minute divergences of measurement. Nevertheless, their heads appear to me to be essentially brachycephalic, as compared with those of other tribes in part displaced by them ; but—in so far as may be judged from the observation of the living head covered with the thickly matted and long coarse hair of the Indian—they are not remarkable for vertical elevation.

It is by no means an easy thing to obtain actual measurements of Indians' heads. I have seen an Indian not only resist every attempt that could be ventured on, backed by arguments of the most practical kind ; but on the solicitation being pressed too urgently, he trembled, and manifested the strongest signs of fear, not unaccompanied with anger, such as made a retreat prudent. In other cases, where the Indian has been induced to submit his head to examination, his squaw has interfered and vehemently protested against the dangerous operation. The chief object of dread seems to be lest thereby the secrets of the owner should be revealed to the manipulator : but this rather marks the more definite form of apprehension in the mind of the Christianized Indian. With others it is simply a vague dread of power being thereby acquired over them ; such as Mr Paul Kane informs me frequently interfered to prevent his taking the portraits of the Indians of the North-West, unless by stealth.

Table I.—Cranial Measurements.—(Chippeways.)

	Longitudinal Diam.	Parietal Diam.	Frontal Diam.	Inter-mastoid Arch.	Horizontal Circumf.
1. Joseph Shilling	7.5	6.1	5.6	14.4	22.9
2. James Inglesol (Kobsequan) ..	7.4	6.	5.	14.8	22.3
3. Jac. Crane (Now-keise-gwab) ...	7.1	6.	5.4	15.4	22.1
4. Peter Jacobs (Pah-tah-se-ga) ...	7.3	5.	5.4	15.	22.6
5. Jacob Shilling	6.9	6.	5.1	14.7	22.
6. William Snake	7.1	6.	5.5	15.1	22.
7. Crania Americana, No. 683......	7.3	5.8	4.8	15.1	20.9
8. Crania Americana, No. 684......	7.2	5.5	4.3	14.8	20.2

The preceding table presents the results of an examination of six pure-breed Chippeways, at the Indian reserve on Lake Couchiching; with the addition of two others, the only examples of the same nation, given by Morton, in the *Crania Americana*. From these it will be seen that, while in the majority of them a certain approximation of the longitudinal to the parietal diameter is discernible, it is of a very partial nature, except in one instance (No. 5), where a manifest correspondence to certain relative proportions of the Mound-builder type of head is apparent.

Some of the measurements in the living head are necessarily affected by the hair, always coarse and abundant in the Indian. Others again, such as the vertical diameter, cannot be taken; but the mastoid processes are sufficiently prominent to leave very little room for error in the measurement of the intermastoid arch; and this suffices to show the very exceptional approximation of the modern Chippeway head—in so far as it is illustrated by these examples—to the ancient type, in the proportional elevation of the vertex. In the horizontal circumference some deduction must be made for the hair, to bring it to the true cranial measurement in all the six living examples.

I have selected the Chippeways for reference here, because—taking the above measurements, along with other observations—they appear to indicate a nearer approach to some of the assumed characteristics of the American cranial type, in this widely-spread branch of the Indian stock, than is observable in other Northern races, and especially than is apparent on an examination of skulls belonging, as I believe, to the original Huron occupants of the greater part of the country around Lakes Simcoe and Couchiching, where the Chippeways more especially referred to are now settled, including Upper Canada when first explored.

But the divergent characteristics noticeable in these, and still more in the crania of older Canadian graves, are by no means confined to those named, as a few examples will suffice to show. Such a radical divergence from the assumed normal type as has been already noted in Dr Morton's selected Cherokee cranium is no less obvious in that of the Miami,—the head of a celebrated chief, eloquent, of great bravery, and uncompromising hostility to the Whites. (*Crania Americana*, p. 182.)

Longitudinal diameter	7.3
Parietal diameter	5.5
Vertical diameter	5.5
Intermastoid arch	14.5
Horizontal circumference	19.8

In the example of the Potawatomies, "A skull of a genuine

Potowatomie, remarkable for its capacity behind the ears." (Ib. p. 186.)

Longitudinal diameter	7.8
Parietal diameter	5.7
Vertical diameter	5.3
Intermastoid arch	16.0
Horizontal circumference	22.1

In that of the Blackfeet, the largest of two brought to Philadelphia by Catlin, and noted by Dr Morton for its great breadth between the parietal bones, it is also very markedly pyramidal. Nevertheless, here also the longitudinal diameter is nearly two inches in excess both of the parietal and vertical diameters. (Ib. 202.)

Longitudinal diameter	7.1
Parietal diameter	5.4
Vertical diameter	5.1
Intermastoid arch	13.8
Horizontal circumference	19.9

So also Dr Morton says of the Menominees: "I have received a series of Menominee skulls, embracing eight specimens. They are something larger than the average of Indian crania; and although for the most part they present a *rather oval shape*, they are all marked by a gently flattened occiput." (Ib. 179.) A reference to the Catalogue of the Morton Collection at Philadelphia discloses the important fact that of those marked by the shorter longitudinal diameter, Nos. 35, 44, and 563, are females.

Again of the Delawares he remarks: "The few Delaware skulls in my possession are more elongated than is usual in the American tribes; they are also narrower in proportion in the parietal diameter, and less flattened on the occiput."

Such are some indications of data,—derived from a source altogether unexceptionable in the present argument,—which seem to render it impossible to uphold the views so repeatedly affirmed, of the physiognomical, physiological, and, above all, the cranial unity characterizing the whole ancient and modern aborigines of the New World.

I omit, meanwhile, any reference to the characteristics ascribed by Dr Morton to the Iroquois and Hurons or Wyandots: those tribes to whom, with the greatest probability, may be assigned the crania specially examined by me, found along the shores of Lake Ontario, the north shore of Lake Erie, and on Lake Huron. When Champlain effected permanent settlements on the Lower St Lawrence in 1608, he found the north shores of the river occupied, below Quebec, by the Montagnets or Montagnards, and above it by the Ottawas, and other branches of the Algonquin stock. The country to the westward, constituting the great Canadian peninsula lying between Georgian Bay, the Lakes Huron, Erie, and Ontario, was chiefly, if not entirely, in the possession of the Hurons ; while the Iroquois—to whom the latter were most nearly allied in social and physical characteristics, though at deadly enmity with them—occupied the south bank of the St Lawrence, and had their chief villages scattered among the clustering lakes, and the rivers, on the southern shore of Lake Ontario, which they continued to occupy and cultivate till driven out or exterminated in the revolutionary wars. The Iroquois and the Huron tribes were alike distinguished from many others, and especially from the neighbouring hunter tribes of the Algonquin nations, by considerable attention to cultivation, and by living permanently in large settled villages. But the Iroquois wars effectually arrested the progress of agriculture, and at length eradicated or drove out the Hurons from their country between Georgian Bay and Lake Ontario, where they were replaced by rude Algonquin tribes formerly lying to the north of them.

The Hurons, then, and in very modern years the Algonquins, but more especially the former, are the occupants of the country immediately to the north of Lakes Erie and Ontario, whose remains are to be looked for in the Indian graves of this district. Of these tribes Latham remarks : "The Iroquois and Algonquins exhibit in the most typical form the characteristics of the North American Indians, as exhibited in the earliest descriptions, and are the two families upon which the current notions respecting the physiognomy, habits, and moral and intellectual powers of the so-called Red Race are chiefly founded."* In many respects, however, they presented a striking contrast. The Algonquin stock, represented by the modern Chippeways, is only known to us as embracing rude and savage hunter tribes; and both physically and intellectually the Chippeways were inferior to the Iroquois and Hurons. The latter displayed a manifest aptitude for civilization. In war they repeatedly effected and maintained extensive and powerful combinations. Their agricultural operations gave proof of a systematic and continuous cultivation of the soil. Corn, especially, was grown to a great extent. Tobacco also was so extensively cultivated by one of the tribes of Upper Canada as to lead to its designation by the French Jesuit Missionaries of the seventeenth century as the *Petunians*, or Tobacco Growers. Moreover, their knowledge and practice of agriculture appear to have originated independently of all European influence; and but for their fatal involvement in the struggle between the colonists and the representatives of the mother country, there seemed a reasonable prospect of such an Iroquois civilization being developed in the western districts of the state of New York, as might have enabled these representatives of the ancient owners of the soil to share in the gradual advancement of European arts and progress, instead of being trodden under heel in the march of civilization.*

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* Varieties of Mankind, p. 333.

Of Indian skulls dug up within the Canadian district once pertaining to the Huron or Wyandot branch of the Iroquois stock, I had observed and cursorily examined a considerable number before my attention was especially drawn to the peculiar characteristics now under consideration, owing to my repeated rejection of those which turned up, as failing to furnish specimens of the assigned typical American head. Since then I have carefully examined and measured twenty-nine Indian skulls, with the following results:—

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* La Hontan estimated the Iroquois, when first known to Europeans, at 70,000. At the present time they number about 7000, including those in Canada; and they still exhibit traces of the superiority which once pertained to them in comparison with other Indian tribes. The very name of a Mohawk still fills with dread the lodges of the Chippeways; and the Algonquin Indians settled on the Canadian reserves on Lake Couchiching and Rice Lake have been known repeatedly to desert their villages, and camp out in the woods, or on an island, from the mere rumour of a Mohawk having been seen in the vicinity.

Table II.—Cranial Measurements.—Western Canada (Hurons.)

		1. Long. Diam.	2. Pariet. Diam.	3. Front Diam.	4. Vertic. Diam.	5. Inter-mast. Arch.	6. Inter-mast. Line.	7. Occip. front. Arch.	8. Do from Occiput to root of nose.	9. Horiz. circumference.
1	Orillia	7.5	5.7	4.5	5.6	15.6	4.25	15.	13.	21.1
2	do.	7.4	5.5	4.4	5.4	14.7	4.5	......	12.	20.6
3	Oakridges	7.6	5.5	4.7	6.	15.7	4.6	15.	13.7	21.2
4	do. (Female)	6.8	4.8	4.2	5.	13.6	4.	13.2	11.3	18.9
5	Windsor	6.6	5.3	4.2	5.5	14.5	4.2	13.5	12.2	19.
6	Peterborough	7.7	5.5	4.9	5.3	15.4	4.6	15.	13.6	21.1
7	Windsor	7.	5.7	4.7	5.7	15.2	4.3	14.5	12.9	20.1
8	do.	7.	5.7	4.5	5.7	16.1	4.	14.4	12.4	20.1
9	do.	7.4	6.1	4.9	5.7	......	4.5	15.5	13.4	21.4
10	Penetanguishene	7.8	5.6	4.6	5.9	15.5	4.5	15.6	13.5	21.3
11	Barrie	6.6	6.4	5.2	5.3	16.	4.6	14.4	12.1	20.7
12	Burlington Bay	7.	5.25	4.4	5.3	14.	4.	13.6	11.9	19.5
13	do. do.	7.6	5.6	4.4	5.4	15.2	4.2	14.9	12.9	20.9
14	Burwick	7.2	5.1	4.4	5.6	14.3	4.3	14.7	12.4	21.
15	Tecumseth	7.3	5.6	4.4	5.5	14.5	4.9	14.4	12.5	20.25
16	do. (Female)	7.2	5.2	3.9	5.	14.1	3.6	14.25	12.9	19.7
17	do.	7.9	6.	4.6	5.7	16.	3.4	16.1	14.2	22.
18	do. (Female)	7.6	5.25	4.3	5.6	14.	4.1	14.25	12.6	20.2
19	do. (Female)	7.5	5.2	4.1	5.1	13.4	4.2	14.8	13.	20.5
20	do.	7.4	5.6	4.6	5.5	15.	4.4	15.	13.6	20.9
21	do.	7.6	5.4	4.2	5.7	15.1	4.4	15.3	14.	20.9
22	Owen Sound	7.	5.5	4.2	5.	13.8	4.	14.	12.2	19.8
23	do.	7.3	5.3	4.25	5.25	14.4	4.2	14.25	12.4	20.4
24	do.	7.2	5.4	3.8	5.25	14.5	3.9	14.2	12.	19.9
25	do.	7.7	5.4	4.7	5.6	14.6	4.2	15.	13.	21.4
26	Oro	7.4	5.4	......	4.25	15.25	4.	14.9	12.4	20.4
27	Owen Sound	7.5	5.9	5.1	5.5	15.	4.25	15.6	13.3	21.8
28	do.	7.6	5.5	4.5	5.4	14.6	4.5	14.9	13.1	21.3
29	Oro	7.5	5.6	4.4	5.5	15.6	4.3	15.2	13.	21.4

1. Only three exhibit such an agreement with the American type, as, judged by the eye, to justify their classification as true brachy-cephalic crania. One of these (No. 11), a very remarkable and massive skull, was turned up at Barrie, on Lake Simcoe, with, it is said, upwards of two hundred others. It differs from all the other Indian crania in exhibiting the vertical occiput so very strikingly, that, when laid resting on it, it stands more firmly than in any other position. Of the Scioto Valley cranium, Dr Morton remarks, in reference to the occiput: "Similar forms are common in the Peruvian tombs, and have the occiput, as in this instance, so flattened and vertical, as to give the idea of artificial compression; yet this is only an exaggeration of the natural form, caused by the pressure of the cradle-board in common use among the American nation." I think it extremely probable that further investigation will tend to the conclusion that the vertical or flattened occiput, instead of being a typical characteristic, pertains entirely to

the class of artificial modifications of the natural cranium familiar to the American Ethnologist alike in the disclosures of ancient graves, and in the customs of widely separated living tribes. In this I am further confirmed by the remark of Dr Morton, in reference to the Peruvian crania: " These heads are remarkable, not only for their smallness, but also for their irregularity; for in the whole series in my possession there is but one that can be called symmetrical. This irregularity chiefly consists in the greater projection of the occiput to one side than the other, showing in some instances a surprising degree of deformity. As this condition is as often observed on one side as the other, it is not to be attributed to the intentional application of mechanical force; on the contrary, it is to a certain degree common to the whole American tribes, and is sometimes, no doubt, increased by the manner in which the child is placed in the cradle."* To this Dr Morton subsequently added the further remark, in describing an unsymmetrical Mexican skull: "I had almost omitted the remark, that this irregularity of form is common in, and *peculiar to, American crania.*"† The latter remark, however, is too wide a generalization. I have repeatedly noted the like unsymmetrical characteristics in the brachycephalic crania of the Scottish barrows; and it has occurred to my mind, on more than one occasion, whether such may not furnish an indication of some partial compression, dependent, it may be, on the mode of nurture in infancy, having tended, in their case also, if not to produce, to exaggerate the short longitudinal diameter, which constitutes one of their most remarkable characteristics. In the case of the Barrie skull, there can be little doubt that the flattened occiput is the result of artificial compression, of a much more decided nature than that of the cradle-board of the Papoose. It is not undeserving of notice here, that the example selected by Cuvier, among his "crania pertaining to the four principal types of the human species," to illustrate the American race, exhibits a strikingly marked prolongation of the occiput. It is described as: " *Crâne trouvé dans une caverne, près du Village de Maïpuré près des bords de l'Orénoque; rapporté*

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* Crania Americana, p. 115.

† Types of Mankind, p. 444

par M. de Humboldt;"* and so far suffices to indicate in how far the opinion already quoted from Humboldt's *Researches* coincides with his own independent observations.

2. In addition to what has been above remarked in reference to the probable artificial origin of the supposed typical form of occiput, assigned by Dr Morton to the whole American race, I am struck, in the majority of the examples examined, with the total absence of any approximation to the flattened occiput. Fifteen of the crania referred to exhibit a more or less decided posterior projection of the occiput, twelve of these markedly so, and seven of them present such a prolongation of it as constituted one of the most striking features in one class of ancient Scottish crania, which chiefly led to the suggestion of the term Kumbo-cephalic† as a distinctive term for them.

3. The tendency to the pyramidal form, occasioned by the angular junction of the parietal bones, is apparent in the majority of the skulls examined. I have noted its occurrence more or less prominently in fourteen crania, of which five exhibit a strongly-marked pyramidal form, extending to the frontal bone. In some, however, it is only slightly indicated, while in several it is totally wanting.

4. I am further struck with the frequency of the very partial projection, and in some examples the total absence of the superciliary ridge, a characteristic which I am not aware has been noted before. In six of the skulls carefully noted by me this is particularly manifest, and, along with their pyramidal vertex and predominant longitudinal diameter, suggests affinities, hitherto overlooked, with the Esquimaux form of skull.

5. I would also note that, whereas Dr Morton states, as the result of his experience, that the most distant points of the parietal bones are, for the most part, the protuberances, I have only found such to be the case in two out of twenty-nine Canadian skulls. The widest parietal measurement is generally a little above the squamous suture.

6. The occurrence may also be noted in several of these crania, of Wormian bones, of such regularity of form and position as to constitute indications at least, seemingly confirmatory of

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* Cuvier: "Le Regne Animal." Races Humaines, planches 1 et 2, pl.8, fig. 2.

† Prehistoric Annals of Scotland, p. 109.

the supposed tendency to the development of an *interparietal*, or *super-occipital* bone, first pointed out by Dr Bellamy. This, which is a permanent cranial characteristic in some of the mammalia, is regarded by Dr Tschudi as an osteological feature peculiar to the Peruvians, and is, he affirms, traceable in all the skulls of that race.

The table of measurements of skulls procured from Indian cemeteries to the north of Lakes Erie and Ontario (Table II.), supplies some, at least, of the elements essential to the formation of a sound judgment on the question under consideration. It embraces twenty-nine examples. To these I have added, in another table (Table III.), the corresponding measurements of the skull of the celebrated Mohawk chief, Joseph Brant (Tayendanaga), from a cast taken on the opening of his grave, at the interment of his son, John Brant, in 1852. I have also further added, from the *Crania Americana*, the Iroquois and Huron examples given there, which, it will be seen, agree in the main with the results of my own independent observations; while a comparison of the two tables will be satisfactory to those who may not unnaturally hesitate to adopt conclusions, based on the amount of evidence produced, adverse to opinions re-affirmed under such various forms by so high an authority as Dr Morton, and adopted and made the basis of such comprehensive inductions by his successors.

Table III.—Cranial Measurements.—Six Nations.

	1. Long. Diam.	2. Pariet. Diam.	3. Front. Diam.	4. Vertic. Diam.	5. Inter-Mast. Arch.	6. Inter-Mast. Line.	7. Occip. front. Arch.	8. Do. from Occiput to root of nose.	9. Horis. circumference.
Mohawk: Brant	7.8	6.	5.	...	15.6?	...	...	13?	22.
Oneida, Morton, No. 33........	7.5	5.6	4.1	5.8	14.4	4.3	14·9	.....	20.8
Cayuga, do. No. 417......	7.8	5.1	4.2	5.4	14.2	4.5	15.5	.....	20.8
Huron, do. (Fem.) No. 607	6.7	5.6	4.1	5.2	14.5	3.9	14.	.....	19.2
Huron, do. No. 15	7.2	5.3	4.3	5.5	15.	4.4	14.2	.....	19.8
Iroquois, do. No. 16	7.5	5.5	4.5	5.7	15.2	4.5	15.1	.....	20.8
Iroquois, do. A.N.S.	7.1	5.4	4.2	5.3	14.3	4.	14.1	.....	20.

The intimate relations in language, manners, and the traditions of a common descent, between those northern and southern branches of the Iroquois stock, render these two tables, in so far as they present concurrent results, applicable as a common test of the supposed homogeneous cranial characteristics of the aboriginal American, in relation to the area of the great Lakes. Twenty-nine skulls, such as the first table supplies, or thirty-six as the result of both, may, perhaps, appear to be too small a number on which to base conclusions adverse to those promulgated by an observer so distinguished and so persevering as Dr Morton, and accepted by writers no less worthy of esteem and deference. Still more may these data seem inadequate, when it is remembered that Dr Morton's original observations and measurements embraced upwards of three hundred American skulls. But—in addition to the fact that the measurements now supplied are only the more carefully noted data which have tended to confirm conclusions suggested by previous examinations, in a less detailed manner, of a larger number of examples—an investigation of the materials which supplied the elements of earlier inductions will show that only in the case of the ancient "Toltecan" tribes did Dr Morton examine nearly so many examples; while, in relation to what he designated the "Barbarous Race," to which the northern tribes belong, even in Dr Meigs' greatly enlarged catalogue of the Morton collection, as augmented since Dr Morton's death, the Seminole crania present the greatest number belonging to one tribe, and these only amount to sixteen.

In contrast to the form of head of the true American race, Dr Morton appends to his *Crania Americana* drawings and measurements of four Esquimaux skulls, familiar to me, if I mistake not, in the collection of the Edinburgh Phrenological Society. In commenting on the views and measurements of these, he remarks:—"The great and uniform differences between these heads and those of the American Indians will be obvious to every one accustomed to make comparisons of this kind, and serve as corroborative evidence of the opinion that the Esquimaux are the only people possessing Asiatic characteristics on the American continent." In some respects this is undoubtedly true; the prognathous form of the superior maxilla, and the very small development of the nasal bones, especially contrast with well-known characteristics of the American aborigines. But having had some little familiarity in making comparisons of this kind, it appears to me, notwithstanding these distinctive points, that an impartial observer might be quite as likely to assign even some of the examples of Iroquois and other northern tribes figured in the *Crania Americana,* to an Esquimaux, as to a Peruvian, Mexican, or Mound-builder type. Compare, for example, the vertical and occipital diagrams, furnished by Dr Morton, of the Esquimaux crania (p. 248) with those of the Iroquois and Hurons (pp. 192-194). Both are elongated, pyramidal, and with a tendency towards a conoid rather than a flattened or vertical occipital form ; and when placed alongside of the most markedly typical Mexican or Peruvian heads, the one differs little less widely from these than the other. The elements of contrast between the Hurons and Esquimaux are mainly traceable in the bones of the face ; physiognomical, but not cerebral.

Taking once more their cranial measurements as a means of comparison; these, when placed alongside each other, equally bear out the conclusions already affirmed. For comparison, I select, in addition to the Scioto Valley Mound-builder, the following, as those pointed out by Dr Morton's own descriptions as among the most characteristic he has figured :—Plate XI., Peruvian, from the Temple of the Sun. " A strikingly characteristic Peruvian Head." Plate XI., C. " Here, again, the parietal and longitudinal diameters are nearly equal. The posterior and lateral swell of this cranium is very remarkable, and the vertex has the characteristic prominence." Of the Mexican skulls, Dr Morton remarks of Plate XVII.—" With a better forehead than is usual, this skull presents all the prominent characteristics of the American race,—the prominent face, elevated vertex, vertical occiput, and the great swell from the temporal bones upward ;" and Plate XVIII.—" A remarkably well-characterized Toltecan head, from an ancient tomb near the city of Mexico." Here it is scarcely possible to avoid the conclusion, that if a clear line of separation must be drawn, it cannot be introduced to cut off the Esquimaux from the others, but must class with them the Iroquois group, though pure Indians, as more nearly allied to them than to the Toltecans or the Mound-builders.

Table IV.—Comparative Cranial Measurements.

	Longitud'. Diameter.	Parietal Diameter.	Frontal Diameter.	Vertical Diameter.	Intermastoid Arch.	Intermastoid Line.	Occipito-frontal Arch.	Horizon. Circumf.
Sciote Mound	6.5	6.	4.5	6.2	16.	4.5	13.8	19.8
Peruvian	6.1	6.	4.7	5.5	16.	4.5	14.1	19.5
„	6.	5.9	4.4	5.	15.5	4.	13.2	19.
Mexican	6.8	5.5	4.6	6.	15.6	4.4	14.6	19.9
Toltecan	6.4	5.7	4.5	5.4	14.6	4.5	13.5	20.2
Iroquois..................	7.5	5.5	4.5	5.7	15.2	4.5	15.1	20.8
Cayuga..................	7.8	5.1	4.2	5.4	14.2	4.5	15.5	20.8
Oneida	7.5	5.6	4.1	5.8	14·4	4.3	14.9	20.8
Huron	7.2	5.3	4.3	5.5	15	4.4	14.2	19.8
Esquimaux	7.5	5.4	4.6	5.4	14.3	4.1	15.2	20.4
„	7.3	5.5	4.4	5.3	14.1	4.3	14.4	20.3
„	7.5	5.1	4.3	5.5	14.8	3.9	15.5	20.3
„	6.7	5.	4.4	5.	13.6	4.	13.9	18.9

These examples I refer to in preference to those presented in the previous table as the result of my own observations, as they are necessarily unbiassed. They are the specimens of the very stock I refer to, selected or brought by chance under the observation of Dr Morton, and included as the characteristic or sole examples of its tribes or nations, in his great work. But the same conclusions are borne out by the examples obtained within the Canadian frontiers; and they seem to me to lead inevitably to this conclusion, that if crania, measuring, in some cases, two inches in excess in the longitudinal over the parietal and vertical diameters, and in others nearly approximating to such relative measurements,—without further reference here to variations in occipital conformation, —if such crania may be affirmed, without challenge, to be of the same type as others where the longitudinal, parietal, and vertical diameters vary only by small fractional differences, then the distinction between the *brachycephalic* and the *dolichocephalic* type of head is, for all purposes of science, at an end, and the labours of Blumenbach, Retzius, Nilsson, and all who have trod in their footsteps, have been wasted in pursuit of an idle fancy. If differences of cranial conformation of so strongly defined a character, as are thus shown to exist between the various ancient and modern people of America, amount to no more than variations within the normal range of a common type, then all the important distinctions between the crania of ancient European barrows and those of living races amount to little ; and the more delicate details, such as those, for example, which have been supposed to distinguish the Celtic from the Germanic cranium ; the ancient Roman from the Etruscan or Greek ; the Sclave from the Magyar or Turk ; or the Gothic Spaniard from the Basque or Morisco, must be utterly valueless. If external circumstances or the progress of civilization exercise any influence on physical form, a greater diversity of conformation is to be looked for in Europe than among the Indians of America, where, as in Africa, nearly the same habits and modes of life have characterized the whole "Barbarous Race" throughout the centuries during which Europe has had any knowledge of them. But, making full allowance for such external influences, it seems to me—after thus reviewing the evidence on which the assumed unity of the American race is founded—little less extravagant to affirm of Europe than of America, that the crania everywhere, and at all periods, have conformed or even approximated to one type.

As an hypothesis, based on evidence accumulated in the *Crania Americana*, the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalizations relative to the Southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the Northern continent, the conclusions still go far beyond anything established by absolute proof; and the subsequent labours of Morton himself, and still more, of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing, by further evidence, a novel and ingenious hypothesis.

Dr Latham, after commenting on the manifest distinctions which separate the Esquimaux of the Atlantic from the tribes of the American aborigines lying to the south and west of them, as elements of contrast which have not failed to receive full justice, adds : "It is not so with the Eskimos of Russian America, and the parts that look upon the Pacific. These are so far from being separated by any broad and trenchant line of demarcation from the proper Indian, or the so-called Red race, that they pass gradually into it ; and that in respect to their habits, manner, and appearance, equally. So far is this the case, that he would be a bold man who should venture, in speaking of the southern tribes of Russian America, to say *here the Eskimo area ends, and here a different area begins.*"* The difference thus pointed out may be accounted for, to a considerable extent, by the diverse geographical conformation of the continent, on its eastern and western sides, which admit in the latter of such frequent and intimate intercourse as is not unlikely to lead to an intermixture of blood, and a blending of the races, however primarily distinct and diverse. The evidence presented here, however, refer to tribes having no such intercourse with the Esquimaux, and distinguished from them by many important characteristics in manners, social habits, and external physiognomy. Nevertheless, if these conclusions, deduced from the examination of Canadian crania, are borne out by the premises, and confirmed by further investigation, this much at least may be affirmed : that a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi ; and in so far as the northern differ from the southern tribes, they approximate more or less, in the points of divergence, to the characteristics of the Esquimaux :—that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent Ethnologists to be physically Mongol and Asiatic, if philologically American.

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* Varieties of Man, p. 291.

On some Modified Results attending the Decomposition of Bituminous Coals by Heat. By Dr A. A. HAYES, U.S.

When bituminous coal is exposed in proper vessels to a gradually increasing temperature, at a certain point decomposition commences and continues, while heavy hydrocarbon vapours, mixed with the vapours of water and salts of ammonia, escape, and may be condensed.

The proportion of permanent gases formed is small in comparison with the weight of the liquids produced, when the decomposition of the coal is carefully regulated.

In the ordinary rapid breaking up of the composition of coal by heat suddenly applied in the manufacture of illuminating gas, the proportion of permanent gases is increased, but the heavy fluid hydrocarbons are also formed. This mode of decomposition is evidently a mixed one, partaking of the characters of a regulated distillation, while at the same moment a more complete destruction of the coal is proceeding in some parts of the mass.

A further decomposition of the fluid products, condensed from either or both of these modes of operating, takes place when we again subject them to the influence of heat; and this well-known fact is the basis on which improvements in the manufacture of illuminating gas have been founded,—a secondary destruction of vapours being effected in appropriate apparatus, heated to a high temperature.

This character, which all the bituminous coals exhibit, of passing into carbon nearly free from vapours only when heavy fluid hydrocarbons are also formed, has, in a chemical view, been the strongest fact adduced in opposition to the generally received opinion that the anthracites and semi-anthracites have resulted from chemical changes of bituminous coal, through the agency of the heat of igneous rocks which have disturbed their beds. The heavy hydrocarbons, represented by ordinary coal tar, are the most indestructible bodies known; and wherever anthracites exist, we should expect to find near by those products of the chemical changes effected in the coal. Such is the delicacy of the balance existing between the elements of the heavy hydrocarbons, that no second distillation of them can be effected; they always undergo decomposition by heat, with the separation of carbon, which, under any known natural conditions, would remain to attest their previous presence.

Considerations of this kind have led me to experiment on the changes which coals undergo by heat, where the influencing conditions were not the same as those usually seen; and the results of extended trials demonstrate that the bituminous coals may be broken up into permanent gases, vapours of water, and ammoniacal salts, while carbon remains as a fixed product.

If we substitute, for the ordinary forms of apparatus used in decomposing coal by heat suddenly applied, any modification of form which compels the gas, as it forms, to escape from the more highly heated part of the mass of coal, through a small opening, or, better, a small eduction-pipe, the heavy hydrocarbons do not form part of the products which escape. Generally the light, nearly colourless, oils of the benzole series, appear with the aqueous solutions of the ammoniacal salts, while only an accidental quantity of carbon is deposited in the eduction-pipe. The carbon left is more than usually compact and hard; and such coals as ordinarily produce much water, when they form heavy hydrocarbons, afford less than half the usual amount, when thus decomposed, under the influence of the constant presence of an atmosphere of permanent gases.

In following the observations at the earlier stage, it was found that the size of the eduction-tube leading the gas from the hotter part of the mass of coal undergoing changes, exerted a most marked effect on the composition of the products. It was established as a fact, that in an ordinary coal-gas retort, the size of the conduit might be varied so as to allow the tar-like bodies to form, or to prevent their appearance at pleasure.

But a more remarkable result was obtained, when, after having prevented the production of heavy hydrocarbon fluids, the influence of reduced size of tube was studied in its relation to the composition of the gas afforded by a particular kind of coal. To a certain extent, the chemical constitution of the gas formed was found to be under control, and the conclusion reached was, that dissimilar permanent gases may be thus obtained from the same parcel of coal without a modification of temperature.

Any explanation of the change of composition induced in the volatile parts of bituminous coals under the above-described conditions should not include mechanical pressure, which is no greater than often exists in ordinary cases.

It seems probable that the presence of an atmosphere of nearly permanent gases in the decomposing vessel, and the regular continuous flow of them from the coal, prevent the formation of heavy vapours at the instant of change in the coal. In support of this point, we find the temperature necessary to convert coal into gas without the presence of heavy hydrocarbons much less high than when they are produced.

We may therefore observe the decomposition of coal without the simultaneous formation of tar, and beds of coal may be converted under existing natural conditions to anthracite, without secondary products being formed.

On Ozone Observations. By Professor WILLIAM B. ROGERS.

In the May number of the *Ann. de Chem.*, M. Cloez describes a number of experiments, from which he concludes that the usual observations for detecting the presence and estimating the amount of ozone in the air are entirely without value. "N'ont aucune espece de valeur." Having for the last two years made daily observations with Schoenbein's test, I have been interested in repeating some of the more important of M. Cloez's experiments, and in making others which seemed likely to elucidate the subject. The results, although in some cases coincident with his, have often differed from them, and have, on the whole, satisfied me, that under the conditions of my observations the influences to which M. Cloez ascribes so much importance cannot materially impair the accuracy of the test.

As indicating the local circumstances of these experiments, it is proper to remark, that they were made in the midst of an undulating rural district, remote from manufactories, about 400 feet above the sea-level, and distant about forty miles from the coast. The soil of the region generally is a sandy loam and gravel, resting on drift and gneissoid rocks, and covered with grass and occasional small tracts of woodland, in which pine is the predominant growth. Nearly all the experiments were made on the top of a grassy hill about one fourth of a mile from the nearest wood.

I. *Of the Effect of the Terebinthinate and Aromatic Exhalations of Plants.*

A number of observations are adduced by M. Cloez to prove that these emanations escaping from plants impart to the air the power of affecting the test-paper in the same manner as ozone, without, as he believes, first ozonising the atmospheric oxygen.

The essence of turpentine, it is well known, acts energetically on the test-paper. I have found that one of Schoenbein's slips, suspended in a phial containing a few drops of this liquid, even in an obscure light, will become sensibly purplish in one minute, and if removed and moistened, will show a decided colouring of its whole surface, augmenting to a strong purple at the edges. If retained in the phial for fifteen minutes, it assumes a tint corresponding to 7 of Schoenbein's scale; and exposed for double that time to the turpentine vapour, it acquires an intense dark purple hue, equal to the maximum of the scale.

To test the effect of various aromatic oils under like circumstances, a series of six ounce phials, furnished with close corks, were charged each with a few drops of a particular oil, and a slip of the iodized paper was suspended within each from the cork. They were placed on a table in diffused light, at a general temperature of 80°, and remained in this position for twenty-four hours. The following were the results:—

(*a*) Oil of Juniper. In the vapour of this oil the paper assumed a brownish tint, with a shade of purple; and when wetted, presented the shade corresponding to about 6½ of the scale.

(*b*) Oil of Sassafras gave nearly the same effect, but sensibly less, about 5½.

(*c*) Oil of Lavender imparted merely a brownish hue to the paper, which on wetting passed into a light purple, marking from 3 to 4 of the scale.

(*d*) Oil of Rosemary produced the same effect as the lavender, but a slight shade more decided.

(*e*) Oil of Pennyroyal impressed brownish stripes on the paper; wetting developed a line of purple at the lower end.

(*f*) Oil of Peppermint did not change the colour of the paper, which also retained its whiteness after wetting.

(*g*) Oil of Cloves. No effect.

(*h*) Oil of Roses. No effect.

(*i*) Oil of Lemons. No effect.

(*k*) Oil of Bergamot. No effect.

(*l*) Oil of Gaultheria. No effect.

The experiment was prolonged for three days with the last six substances without any perceptible action on the paper, except in the case of the oil of peppermint and lemons, the fume of which gave an effect amounting to about 2, and the latter about 3 on the scale.

On making the same experiment with camphor, a like negative result was obtained.

On repeating these experiments under the direct action of sunlight, with the air at about 82° Fahr., the test was affected by all the vapours above enumerated, excepting that of camphor, which, even after several days, was found to be entirely inoperative. The great acceleration of the action caused by the direct rays of the sun will be apparent from the following statement of the effects upon the test, produced by an exposure in sunshine for thirty minutes:—

(*a*) Oil of Juniper. Paper deeply purple, and when wetted almost black, corresponding to 10, the maximum of the scale.

(*b*) Oil of Sassafras. Effect nearly as great as in the Oil of Juniper.

(*c*) Oil of Lavender. Paper became streaked with purplish brown; when wetted, strong purple, in places, amounting on average to 6½.

(*d*) Oil of Rosemary. Light purplish, and when wetted became deeply purple,—equal to 7.

(*e*) Oil of Pennyroyal. Brownish, and when wetted, purple, amounting to 4.

(*f*) Oil of Peppermint, like the preceding.

(*g*) Oil of Cloves. Intense purple, becoming, when wetted, almost black (10).

(*h*) Oil of Roses, purplish-brown, and when wetted, about 2.

(*i*) Oil of Lemons. Light purple, becoming, by addition of water, about 4½.

(*k*) Oil of Bergamot, about 5.

(*l*) Oil of Gaultheria, scarcely a perceptible change.

In order to test the influence of the aroma as it proceeded directly from the plant, I inclosed in a series of bell-glasses, with a little pure water beneath, fresh cuttings of pine, thuja, tansy, and mint, and flowers of the syringa, rose, and odorous yellow lily. A slip of test-paper being suspended in each, they were placed on a table in an obscure light. In all the vessels the characteristic odour of the plant was evolved. Each was inspected twice a-day, to mark any change which might occur in the paper, which, being moistened by the vapour, would at once show even the slightest development of iodine. At the end of 72 hours they were taken out for examination, and gave no evidence of having been acted on by the terebinthinate and other volatile matters. This experiment was repeated several times with the same result. While the observation was in progress, a slip of test-paper suspended daily in the large open hall where the experiments were made, showed an effect varying from 6 to 9 on the ozone scale.

As these exhalations were much more concentrated than they are ever likely to be, either in a grove of pines or a garden of aromatic flowers, it is difficult to conceive how, under ordinary conditions, they can produce a marked impression upon the ozone test. Since, however, the concurrence of strong light has been found greatly to favour the action of the vapours of many essential oils, experiments were made with cuttings of thuja, pine, and tansy, in covered glass jars, placed in direct sunlight. The weather during these experiments was warm, and the sky clear. A parallel experiment was at the same time made by suspending a test-paper in another vessel similarly placed, containing a small quantity of water. In the first series of observations, the exposure to sunlight lasted nine hours. The paper in the jars containing the cuttings became of a light brownish-yellow on the side next the sun. When taken out, they had the odour of the plants, but on being moistened, showed no trace of purple colouring. On repeating the experiment twice under the same conditions, the paper still failed to give any distinct evidence of ozonic influence. In all these observations, however, the papers which had been solarized in moist air simply, besides presenting the brownish-yellow hue on the side next the light, showed, when wetted, a trace of purple, and in one case an effect amounting to about 1½ of the ozone scale.

It would thus appear that a moist atmosphere, combined with direct solar light in a close vessel, has the effect of setting free a minute portion of the iodine of the test, and that in these conditions, the terebinthinate and other odours, instead of favouring, rather counteract the effect.

II. *Of the Effect of Oxygen liberated by Growing Plants.*

It has been asserted on the ground of various experiments, that the oxygen set free by the leaves of plants, when exposed to the sunlight, affects the test-paper, and is to a certain extent ozonised. This is denied by M. Cloez, who, in support of his negative, adduces several experiments to show that the effect in question is due to the united action of moisture and the sun's rays upon the test.

It has been seen, under the preceding head, that the test-paper exposed in moist air to the action of direct sunshine is, in the course of eight or ten hours, sensibly impressed as if by a faint action of ozone. That the heating rays are not the cause of this result, is proved by the entire absence of any change when we cover the glass vessel with an impervious black cloth; and that the presence of moisture is necessary, is proved by the unchanged whiteness of the test when exposed in dry air to the sun, even for an entire day. How far the effect may depend on a change in the starch of the test remains to be determined.

M. Cloez suspended in a bell-glass, containing growing plants, two test-tubes, one naked, the other covered so as to be impervious to light, and each containing a slip of test-paper; and, after exposing these for many hours to the sun's rays, he observed that the paper in the covered tube suffered no change, while that in the other was sensibly affected. On repeating this experiment a number of times, I found that the paper in the tube wrapped in black cloth retained its original whiteness unimpaired; while the other, after the lapse of a few hours, became of a light yellowish-brown on the side next the light, as in the experiment formerly described, the colour, however, being in this case a great deal more faint. When moistened, this paper gave either none, or a doubtful trace of purple. A third paper, suspended freely in the vessel, assumed a much stronger brownish hue, and, when moistened, presented a faint but quite discernible shade of purple.

I next placed three bell-glases, of about one gallon measure, and containing each a suspended slip of the test-paper, in the following conditions:—

(*a*) Over a clump of luxuriant grass, on a wide lawn, the edge of the glass pressed down as closely as possible.
(*b*) Over bare earth on which the grass had died.
(*c*) Over a shallow pan of water.

The vessels were within a few feet of each other, and were exposed throughout the day to the full action of the sun.

On examining the papers a little before sunset, they were all found to have contracted, on the side next the sun, a brownish yellow tint, and on moistening, assumed a light purplish hue, amounting in (*a*) to about 1½ of the ozone scale, in (*b*) to about 1, and in (*c*) the same as in (*a*). This experiment was repeated a number of times with little variation of effect. The paper exposed to the vapour simply, showed in nearly every case as much effect as that exposed to the green grass. When, from the dryness of the bare earth in (*b*), the moisture exhaled into the vessel was comparatively small, the impression upon the paper was less marked. A like experiment was made by placing the first jar over a clump of luxuriant clover, and again over a young poppy plant, but without obtaining any greater effect than with the simply moistened air.

It should be remarked, that during all these observations the effect of the general external air upon the test-paper exposed for the same time ranged from 7 to 10 of the ozone scale.

These results are evidently not favourable to the view which has been maintained—that the oxygen evolved by growing plants is ozonised, and would seem to confirm the opinion of M. Cloez that the combined action of aqueous vapour and sunshine produces the result.

In order to test the effect of the terebinthinate exhalation, as it expands in the open air from plants, the following experiments were made:—

1. A slip of sensitive paper was suspended in a very thick hedge of thuja, and another at the distance of 200 feet in a very leafy Persian honeysuckle, which had been for some time out of bloom. Both papers were entirely shaded from the direct rays of the sun. The temperature throughout the day was about 70°; the sky was occasionally obscured by clouds, and the air in gentle motion. In the hedge of thuja the characteristic odour of the plant was very decided. The two papers, inspected from time to time, were found to show coloration about the same time, and to deepen in tint sensibly at the same rate, so that when examined, after ten hours' exposure, they presented, as nearly as could be estimated, an equal depth of shade, corresponding to about 7 of the ozone scale. A third paper, exposed during the same time in a spot removed from shrubs and trees, gave precisely the same result. This experiment was twice repeated, without showing any marked superiority of effect in the thuja over that of the other two exposures.

2. Papers were suspended in a thicket of larch, pine, and thuja, and in a large leafy lilac, so as to be shaded from the sun. When examined, after eight hours of exposure at a temperature of about 80°, they presented very nearly the same purplish tint, the paper in the lilac being uniformly coloured, the other irregularly striped. When moistened, the latter showed a slightly deeper purple on part of its surface than the former. Taking the deepest tint of each as a measure I estimated the effects as about 5½ and 5 on the scale.

3. A slip of the test-paper was suspended in a shaded spot in a pine wood of several acres extent, at a distance of about ¼ mile from the thicket of thuja, pine, and larch before mentioned, in which another paper was at the same time exposed. The day was remarkably clear, with a gentle breeze from south-west; the temperature ranging from 75° to 85°. A decided terebinthinate odour filled the air of the wood. At the expiration of nine hours the two papers presented, as near as could be judged, the same tint—about 7 of the ozone scale; and a third slip, suspended in the shade, near

the house, during the same time, showed a colouring sensibly, but very slightly, less intense, estimated as 6½ of the scale.

4. To test the effects of moderate exposure under these conditions, papers were suspended at 6 P.M., in the hedge of thuja, in the pine thicket, in a clump of common lilac, in the Persian honeysuckle, and in a shaded place in the open air. In an hour all the papers had begun to change, and were equally affected. The night was clear, with a very gentle breeze from south-west. On the following morning, at 7 o'clock, the papers were found very deeply tinged; that in the open air was decidedly more coloured than either of the others; and that which had hung in the centre of the lilac more than either of the others exposed to plant exhalations.

From these observations, it would seem to follow, that the influence of terebinthinate or other odours, as they are naturally exhaled from plants, is either entirely insensible, or so slight as not to interfere with the accuracy of the test as a measure of atmospheric ozone.

The decided effect upon the test-paper of moisture and sunshine combined, as shown in the experiments previously cited, leaves no doubt that in the free atmosphere these agents must tend to liberate a portion of the iodine, although in less amount than in the bell-glass, where the air is saturated, and the solarization complete. That these agencies, or the supposed ozonising influence of vegetation in sunlight, are practically of little or no influence, is shown by the fact, which I have uniformly observed, that during the night hours the ozonic effect, as measured by the test-paper, is greater than during the day, and that often in dark cloudy weather it is greater than during periods of brilliant sunshine. As illustrating the comparative energy of the effect at night and during the day, the following results may not be without interest.

The observations were made in the positions already described. The night-time extended from 9 P.M. to 9 A.M; the day-time from 9 A.M. to 9 P.M.:—

In June 1856. Ratio of night to day,	100·84
July	100·87
Aug.	100·89½
Sept.	100·82
Oct.	100·83

The Geography of Diseases in the Climates of Peru.

By Archibald Smith, M.D.

Peru is naturally divided into three physical regions, familiarly named by the natives, the Coast, Sierra, and Montaña. Let us define their limits, as recognised in Peru.

1st, " La Costa," or the Coast, stretches between the base of the Andes and shores of the Pacific. It is for the most part a continuous arid desert, intersected only here and there by fertilizing mountain-streams and rivers, many of which, though small in volume during the dry months on the Cordilleras, swell to a large size when it rains on the mountains. Thus, the Rimac at Lima often becomes a formidable river during the dry season on the Coast, when it rains torrents on the Andes. The breadth of this strip of desert between the mountains and sea rarely exceeds twenty leagues, but it extends the whole length of the coast for 1500 miles.

2d, " La Sierra," or Andine division, comprises a wider region, extending from the belt where natural herbage commences on the western, to the furthest summit of the eastern Cordillera chain. It thus embraces all the valleys on the Pacific side of the Western Andes above the level of 7000 feet, and also includes the whole of the plains, hills, and valleys between the double Cordillera chains. From a little below the crest of the eastern mountains we have " La Ceja," or *brow* of the Montaña; and here begins the

3d, Geographical Division of Peru. This fertile region is called " La Montaña," from the Spanish word " monte," which means a wood, thicket, or forest. Descending from the eastern Cordillera crest eastward, for a few leagues only, we come upon that warm and steaming woodland, which is lost in the Brazilian territory, and contains the head-streams of Peruvian river navigation, flowing into the great bed of the Amazon.

I shall now proceed to give a rapid outline of the climates and diseases of the Maritime, Andine, and trans-Andine territories which have been here defined.

Of the Climate of the Coast of Peru, from 3° 30′ *to* 21° 30′ *South Latitude.* Summer maximum heat 80° to 84°, and winter minimum 60° to 64°, Fahr.*—According to the popular use of the name "Costa" in the above territorial regions, it includes various gradations of climates, from the open cultivated valleys of the lower plains to the headland breaches in the rainless zone of the mountains, up to the boundary-line of the Sierra. But we shall first consider the climate and diseases of the coast proper, and afterwards make some observations on the higher rock-bound coast valleys, beyond the reach of seasonal vapours and mists.

It is well known that it seldom or never rains on the coast of Peru. From May to November it is visited by sea-vapours carried on the prevailing winds from west and south-west. Towards the end of June these vapours become more dense and abundant, and produce luxuriant vegetation on the sand-hills of southern and central Peru, to the elevation of about 1500 feet; or, in remarkable years, 2000 feet, perhaps, in some favoured corners into which the vapoury breezes "blow home." From the southern tropic to the northern limit of Peru, close on the equator, there is a progressive diminution of atmospherical humidity. But why does it not rain on this coast as on the contiguous Sierra? Were this the effect of the arrest of the trade-winds by the eastern Cordillera range, we would expect more uniformity of result; for why should it rain on the coast of Guayaquil, and not at Payta, if the barrier of the Andes were the real cause of the phenomenon? Are not the Cordilleras of Quito as lofty as those of northern Peru? In Chile on one hand, and in Ecuador on the other, it rains at the same season of the year on mountains and coast, quite contrary to what happens in Upper and Lower Peru.

I have often been struck by the precision of the *garua* or mist-line of the coast, as well as of the *rain-line,* which the

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* This is the temperature at Lima. In the northern province of Piura, the range of thermometer will be about 10° higher; as it will also be in the neighbourhood of Palpa and Nasca, from 75 to 85 leagues south of Lima. In the orchards of the higher part of this capital near the hills, the night temperature sometimes is observed to be under 60°. The range of barometer in Lima is only from 29$\frac{9}{10}$ to 29$\frac{1}{2}$.

Peruvians take as the natural boundary between that region and the Sierra.

The lower limit of the craggy mountainous zone, which neither rain from the Sierra nor *garua* from the seaboard ever reach, is marked by a wall of fog which skirts the foot of the Andes in the wet season. This wall of mist, or fog, is always seen at a distance hanging over the headlands of the lower plains, as one descends from the Sierra at that time of year; and the higher boundary is in like manner clearly and visibly defined, as you ascend to the Sierra from the coast during the rainy months in the Andine heights. On both the roads from Lima to the Sierra, the one by the valley of Chillon, and the other by the valley of the Rimac, this limit is observed;—on the former from the hamlet of Huaramayo at the foot of the Paxaron, 20 leagues from the capital; and on the other at the village of Surco, 15 leagues from the same. But this difference in distance is not to be considered as indicating a relative diversity in the slope of the Andes at these points. The true reason is, that from the capital, by the Surco road, the ascent begins at once, whereas by the Huaramayo route you first have to ride over an easy plain of six leagues to Caballero northward, before you there begin the regular ascent through the rugged and narrow valley, down which the river Chillon works its winding way from the Cordillera pass of the Viuda. In my time, an octogenarian occupied one of the little lichen-thatched cottages at Huaramayo, and on an evening in January I was one of many Englishmen who stood by the old man's door as it rained in torrents on the Paxaron, only a few hundred yards above our heads; but he assured us that in fifty years that he had resided there, a shower of rain had never reached his cane and wicker mansion!

Diseases of the Coast.—The effects of the climate of the Peruvian Coast are different, on the different races of the inhabitants. Some are of white parentage, but the greater number are a mixed race of all grades between the Indian and African, as well as between the White and Indian, and the White and Negro. The dark family thrive the best in general, but a laxity and weakness of habit is prevalent. The European degenerates in his physical development and muscular vigour, and, though intellectually quick-sighted and ready, morally there is want of firmness and energy.

It has been well observed by Unanue,* that "resfrio," or chills and checks to the perspiration, from even slight changes of temperature, is the most usual exciting cause of such frequent diseases as catarrh, diarrhœa, rheumatism, and even the intermittent and remittent fevers of the Peruvian coast. The Negro race are less liable to ague than the White man or Indian; but at the same time they are prone to intractable cutaneous diseases, intestinal hæmorrhage and diarrhœa, dysentery of the worst form, chronic hepatitis and splenitis, with hypertrophy of the heart, and aneurisms.

The Indian race of the Sierra, *when they come to the coast*, are peculiarly liable to tercianas and dysenteries. In the northern provinces, however,—where the population is chiefly Indian, and the temperature higher than on the shores of the central departments to the south of Santa,—terciana is much less severe than on other parts of the coast. The plain of Tacna, likewise arid and waste for want of water, has a climate free of malaria; and the town, 17° 10′ S. lat., which is seven leagues inland from the sea, and fourteen leagues from its port of Arica, is so comparatively healthy as to be a place of resort to the people of the port and neighbourhood during the *terciana* or aguish season, which, over all the coast, is about the vernal and autumnal equinox.

During the hot months of January and February the irritability of the whole system is increased, particularly of the mucous membrane of the alimentary passages, and thus gastric and bilious fevers readily originate; and cholera morbus also becomes an exceedingly common disease, for which the standing and most efficacious remedy is ice. The native of the coast, no matter of what sex or race, generally shows an early predisposition to habitual *congestive diseases*, such as that most common affection called "*almorrana*," or hæmorrhoids, the plague of the great mass of people above thirty years of age—blennorrhœa, even in tender childhood; uterine, intestinal, and pulmonary catarrh, which are often the preludes of cancer uteri, dysentery, phthisis pulmonalis, asthma, disease of the heart, and dropsy.* Uterine hæmorrhage is a calamity to which the White mother is peculiarly subject, and but for the powerful agency of ice, and iced applications, would be ten times more fatal than it usually is.

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* See his "Observaciones sobre el Clima de Lima;" a treatise which has received the approbation of Baron Humboldt, "Essai Politique," vol. i. p. 350.

Of the diseases of the nervous system, epilepsy, tetanus, and hysteria, are of common occurrence in grown people, and convulsions most fatal in childhood. In protracted fevers of juvenile patients worms are almost always parted with. The more vehement forms of remittent fevers appear in the height and decline of the summer heat. In these there is a more than common tendency to strong determination of blood to the head, and consequent sudden death. The experienced native practitioner knows this, and by prompt measures, and bleeding in particular, endeavours to procure an intermission, or abatement of febrile intensity, to admit of a sixteen or twenty-grain dose of quinine all at once, otherwise another accession, after a few hours, would finally decide the patient's unhappy fate.

The most novel feature in the medical history of Peru now comes to be mentioned. *Yellow fever* was never seen as an epidemic on its shores till towards the close of 1851, when it appears to have been introduced by a ship's cargo of German immigrants landed at Callao. The unfortunate wanderers touched at Rio Janeiro on their outward passage, and there took up the germs of this fearful disease, of which some were ill at sea. On their arrival at Lima, it was observed that unusual mortality attended their fever, much beyond what would have been expected from the symptoms; for, under apparently very moderate febrile action, the sudden transition from seeming convalescence to death was the wonder of all that witnessed it. This disorder, but in a milder form, soon spread in Lima with the increasing heat of summer, and became general in February and March 1852, so that by the end of May scarcely one family escaped in the city or its environs. Yet, with the exception of the German importers, it was not a fatal epidemic. It disappeared as the wet season became established, but again returned with greater strength, and in a modified shape, in the dry season of 1853, by the end of which it showed what we had to expect from it in the summer of 1854, when it proved very fatal in its worst form of "black vomit." In 1855 the season was more advanced before it resumed its energy, and then it carried off with preference many foreigners, English, and others. It is usually mentioned as a characteristic of yellow fever, that it only attacks the same individual once in a lifetime. This may be true where the disease has been long endemic, as at New Orleans, and different parts of the West Indies; but in Peru its importation was so recent, and its progress from infancy to maturity was so gradual and well-marked, that there can be no doubt of the fact that hundreds, probably many thousands, of people experienced a repetition each year of its progress; the earlier stage or type, not protecting from the second or the third. Many who had it in its first form of 1852 escaped the following year, but were severely attacked in 1854. This was my own case. This pestilence also fell with unequal effect on the different races,—Negro, Indian, and European,—in each of whom the therapeutic agency of the same remedies was modified by caste.*

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* See communication from me, dated Lima, 13th May 1848, titled On Phthisis and other Diseases of Lima, in No. xlvi. of the "Medical Examiner and Record of Medical Science," published in Philadelphia, U.S., October 1848.

On the Climate and Diseases of the Rainless Inland Breaks, or "Quebradas," of the Coast.—Looking eastward, and following the course of the rivers from the lower plains to the elevation of 7000 feet on the western slope of the Andes, we pass through this portion of the coast region. The "garua" around Lima rarely rises above 1500 feet in such quantity as to produce vegetation near the foot of the actual body of the Andes, where the spacious valley of the seaboard narrows into a mere ravine, bounded in by craggy precipices. Of course little population and cultivation can exist in such confined and arid space, in many places barely sufficient for the bed of the mountain torrent, by the edge of which the slippery mulepath is cut out of the solid rock. Here and there, however, the rocks recede, and leave a little space for hamlets or villages, and every available bit of soil is cultivated with care. The fruits of the coast are still grown in these spots, which, near the rain-line, as at Surco and Huaramayo, for instance, enjoy an agreeable equable climate, alike removed from the chills of the Sierra, or extreme heats of the coast. In the middle of the day, indeed, when the sun is vertical, these "*quebradas*" are excessively hot, but they are soon tempered by sea-breezes from below, and snow-winds from above.

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* See article titled "Rise and Progress of Yellow Fever in Peru, by Dr Archibald Smith," in the "Edinburgh Medical and Surgical Journal," vol. lxxxii., No. 203, 1st April 1855.

The octogenarian of whom I spoke as the occupant of one of the cottages at Huaramayo, on the confines of the rain-line, was a shoemaker in Lima when young, but having been attacked by hæmoptysis in the capital, was cured of it by the climate of this "cabezada," or headland of the coast valley of Chillon. He repeatedly returned to Lima, and just as often, after a short stay, the hæmoptysis returned; and the end of it was, as a matter of self-preservation, he took up his permanent abode there, and enjoyed, when I saw him, a nimble as well as cheerful old age. Such instances would be endless, if we sought for them, on either side of the extreme limits between Sierra and Coast. Tercianas are common in these ravines, especially at the junction of mountain streams from either side of the pass with the main river. These overflow the banks, leaving slime, which, under the influence of the sun, engenders malaria. The only diseases allowed to be peculiar to these parts are the uta and verrugas. The former exists at Santa Ullaya, in the vicinity of Lima, and Santa Rosa de Quibe and Yangas or Llangas on the River Chillon. It is of the nature of a lupus ulceration, and often incurable. The verrugas, or warty eruption so called, constitute a painful disease, and are said to arise at Yaso (which may be 5000 feet elevation), from drinking a clear crystalline water which jets from the rock; at Surco also, on the other road from Lima to the Sierra, it is not uncommon at an elevation of about 7000 feet. In other localities of the more inland coast range of climate these two diseases are said to prevail, especially in the district of Cajatambo.

On the Climates of the Sierra.—It is hardly possible to give an idea of the climate without giving an outline of the country. I would therefore briefly notice, that on ascending above the rain-line, a sparse vegetation first appears, which gradually improves at every step, until the valleys of the Sierra open out into wide pasture-lands up to the snow-line, with many villages and farms all over the ridges and hollows of the mountains.

When the western Cordillera is crossed, the descent leads into a wide and open district of treeless undulating surface, covered with flocks and herds of sheep, llamas, vicuñas, horses, mules, and horned cattle, &c. But from these lofty and inclement grounds, valleys of different depth and temperature dip off in all directions, making the Sierra, as a whole, a region of the most varied climate and production. As a central point of Andine climate we may take Cerro Pasco as an example. It stands on the verge of the plain of San Juan and Bombon, lat. 10° 36′, and on its northern extremity its mines are drained off into a valley which, by a rapid decline, leads to the milder climate of Quinoa, three leagues below, and thence to the vale of Huanuco, the lower part of which is under 6000 feet of elevation.

By barometrical measurement this mining town is 14,000 feet, or, as Rivero makes it, 14,279 feet above the sea-level, and about 1000 feet above the adjoining plains, being on rising hilly ground in the very midst of the silver mines. The wet season is from November to May (just the contrary to what it is on the coast), but December, January, February, and March are the more disagreeable months, the streets being then all wet and slushy. The weather varies extremely, not merely in the course of the same day, but within a very few hours, during which there may be rapid variations of snow, rain, hail, and sleet, gleams of sunshine, high and fluctuating gusts of wind, sudden obscuration from dense clouds drifting in the atmospheric currents, with flashes of lightning, and peals of thunder rolling among the mountain peaks. For the other six months of the dry season, showers of hail, snow, or rain, are only occasional and rare, the prevailing weather being sunny and cheerful by day, and dry and frosty by night. The thermometer of Fahrenheit I found to be about 42° by day, and 36° by night, in the wet season, but in the dry season the night indications fell to 30°, or under.*

On the Diseases of Cerro Pasco, and other Andine Heights above 12,000 *feet.*—In stating the general pathological effects that force themselves on our notice at the higher levels of permanently inhabited places on the Andes, I would draw attention to the general physiological fact that the Indian of such localities is furnished with far more capacious lungs and fulness of chest, in proportion to his size of body, than the native of the coast; and being thus admirably constructed with a due fitness and relation to the tenuity of the atmosphere in which he lives, he is no more troubled with breathlessness on his native soil than his neighbours of lower regions are on theirs. Our English miners and officials whom I accompanied to Cerro Pasco in 1826 all experienced oppression of breathing, more or less, as they rose above 12,000 feet, by the mountain called the Viuda. In the shade the air was felt to be exceedingly penetrating and chilling, and all the more so when wet in the saddle, the extremities gradually became cold, and the skin shrunk; so that with the consequent repulsion of the cuticular current, and rarefied atmosphere combined, headache and sickness at stomach came on; thus exhibiting what, in the Quichua tongue, is called *zeroche*, or the sickness of the *Puna*. At the more elevated Andine breaks, or full three miles above the level of the sea, the bronchial tubes of the unacclimated suffer severe irritation from the freezing night-air of the dry season. In the trying climate of Cerro and its adjoining plains, the stranger finds his circulation and respiration accelerated on slight exertion; and the lungs, congested, seek relief in occasional deep inspirations;

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* Herndon, on the authority of Rivero (long Prefect of Pasco), says, that during the (dry) months of July, August, and September, the mean temperature of Cerro Pasco is 44° in the day, and 35° at night. The same lamented officer and distinct writer observes in regard to the colour of the atmosphere at Palcomayo, 10,539 feet above the level of the sea: "The sky, at twilight, looked white or grey rather than blue; and I thought it was cloudy until my eye fell on the young moon, with edges as distinct and clear as if it were cut out of silver, and near at hand."—*Exploration of the Valley of the Amazon*, p. 96.

for the diminished pressure of the atmosphere appears to permit a greater expansion of the fluids in the deeper-seated organs, in proportion as the external cold drives the contents of the cuticular vessels back on the great centres of circulation, exciting them to increased action. Nor is it the cold air alone which causes resistance to an equal circulation, by its constringing effects on the dermal and pulmonary capillaries; but the air-cells themselves may be supposed to suffer contraction, under an unusually light atmospheric pressure. In this way, a preternatural distention and irregular action of the arterial, and a turgid condition of the venous system evidently take place. The mountaineer's nimble step and ruddy countenance, indeed, bespeak a free and vigorous circulation; but the purple cheek, red and turgid eye, and livid lip, of the old Spanish miner, tells as surely of his exotic origin as does his skin or speech. Where perspiration is difficult, recovery from disease is also proportionally difficult, whatever be the colour of a man's complexion. This the native knows well; and from the cold table-land he escapes to the subjacent climates, or seeks the natural warm baths of the temperate valley, to sweat off his aches and ailments. The White mother, too, seeks the temperate climate for her confinement, and rearing her tender offspring; for in such elevated stations as Cerro Pasco, white infants are rapidly carried off by croup or convulsive cough. But it is not so with the offspring of the Indian mother of the Sierra, and it is wonderful how little subject she is to uterine hæmorrhage, so alarming in the climate of the Coast; and it has been often remarked that during the long and arduous marches on the Cordilleras in time of warfare, the Indian woman who followed the camp, and happened to have had a baby, was never known on that account to lag a day behind.

It will be readily conceived how the effects of a cold and rarefied atmosphere must aggravate inflammatory affections of the respiratory organs (especially pneumonia), necessarily of frequent occurrence in those inclement regions, and predispose to hæmoptysis and hæmatemesis, as well as hepatic congestion. Cerebral congestion is the common attendant of remittents and intermittents, of which I have seen many instances in Cerro. They showed themselves sometimes so late as twenty days after the patients had left the coast, or place of malaria. When complicated with the *Puna* sickness the aguish element does not at once manifest itself, and the proper treatment in some such cases I have found obscure until remissions or intermissions in the shape of periodical headaches, or otherwise, divulged the secret, when all yielded to the usual antiperiodic remedies.

The pastoral population of the *Puna* appear to be a strong and healthy people. I have observed that the shepherd is careful not to sit in his hut with wet feet, but changes stockings as he draws to his turf fire: for coal is only used in Cerro Pasco, close to which there are abundant beds of it, on the treeless plain. The miners, too, clothe themselves very warmly; but their employment always exposes them to sudden transitions of temperature, especially when they leave the mine at midnight. This leads to rheumatism, and sometimes to what they call "aire," or a slight degree of paralysis, generally removed by a warm and sweating regimen. "*Pechugera,*" or chronic bronchitis, is the natural infirmity of advanced years at the mines. Dyspnœa, is the usual disorder of new comers; but asthma is, as far as I have seen, by no means a prevalent disease on the Andine heights, though one of the most common and distressing on the coast.

Upon what foundation some writers state *cretinism* to be a disease of the Cordillera, I am unable to comprehend. During a continued residence of from four to five years in the Sierra, and nearly twenty years altogether in Peru, I never myself saw, and never, directly or indirectly, in my intercourse with others of all professions and appointments, heard that this disease was known, and much less endemic, in the Cordillera.

Climates and Diseases of the Sierra under 12,000 *feet.*—This is the most thickly peopled range of country in all Peru. In the south, we have Cuzco, the old capital of the Incas, at the elevation of above 11,000 feet, and the department to which it belongs is chiefly peopled by Indians, as is also that of Puno in the cold neighbourhood of Titicaca.* But Arequipa, situated (16° S. lat.) on the confines of Coast and Sierra, at an elevation of between 7000 and 8000 feet above the Pacific (2704 Spanish yards according to Rivero), is peopled chiefly by Mestizoes. This important city (whose fields are ever green by irrigation) shares as much of the temperature of the headland valleys of the coast (already pointed out) as it does of the Sierra. The water of this place is, however, injurious to strangers, producing dysentery, unless previously boiled. It comes from the volcanic heights which surround this oasis in the desert, and no doubt holds in solution some saline matter derived from beds of lava and burning mountains. The whole inter-Cordillera temperate valleys, from south to north of the republic (including the fine central districts of Tarma and Jauja, at the elevation of from 9700 to 10,000 feet and upwards), contain rich grain and arable ground, interspersed with many hilly pastoral ridges and dales, having a medium reigning temperature of 60° Fahr., with a mixed Mestizo and Indian population. Several of the central Andine valleys dip below the level of the Sierra boundary of 7000 feet, marked on the western slope. These produce tropical fruits and sugarcane, and in some instances are infected with malaria and severe terciana,—as, for example, in the glen of Huancabamba, in Huamalies, and also in the vicinity of Cuzco.

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* Titicaca is situated on the frontiers of Bolivia, at the height of 4545 Spanish yards. This lake is 150 miles long by 70 broad at its greatest breadth.

The almost rainless climate of Huanuco reminds one of the rainless valleys of the coast at the same elevation, and extends over a gradual descent of 1000 feet from Ambo to Valles, where the Huallaga, at the level of 5600 feet, or thereabouts, penetrates the break between this Sierra valley on one side and the Montaña vale of Chinchao on the other. Here again recurs the rain problem, of which I made mention in reference to the coast climate. Why should the hills that surround the city of Huanuco be arid, like those of the same level in the coast region, when it enjoys the full benefit of a daily breeze up the river, which sets in regularly about eleven A.M., and continues till three P.M., and is only separated from the

It has many islands and promontories, which produce abundant pastures, potatoes, and other roots of an edible sort of this family, such as the oca, maca, and mashna, as well as a nutritive seed called quinoa. The oca and its varieties, if introduced to England, might supply the place of our potato. See Ledesma's "Corografia del Peru."

Montaña by a comparatively low ridge of the eastern Cordillera? Were the whole inter-Cordillera territory sunk to the same level as the strath of Huanuco, is it not evident that in that case the rainless belt would equally appear on each side of the western Cordillera range, at the elevation of from 7000 feet downwards?

Goitre is the principal endemic of this dry climate, and is also a disease well known in the corresponding sugar valleys of Huarass on the western slope of the Andes. Terciana does not exist in Huanuco; but hepatitis, diarrhœa, and dysentery, especially in the chronic form, are frequent, and cholera morbus in the hotter months of the year. Here there are no violent vicissitudes of temperature. The nights are always delightful, and the sky is almost always calm and clear; for it seldom rains, and when it does, it is but a passing shower. The thermometer (Fahr.) rarely is seen by day above 72° in the shade, though (I think) I have seen it 140° in the sun; and at night I have scarcely ever seen it, during three years, below 66° by the night thermometer in-doors. With so equable and moderate an in-door temperature, the speculative physician might think this one of the finest climates in the world to send his phthisical patients to. Not so, however. In the strath of the valley the consumptive invalid may at first be enlivened by the calm beauty of the scene, but after a protracted residence, is sure to become worse; and the hæmoptic patient in particular would soon succumb, did he not remove to a higher elevation, which he can easily do along the flanks of the hills that bound the valley; but, properly speaking, he is then no longer in the climate of Huanuco, but in that of the temperate Sierra. In the short time and space implied in a three hours' walk up steep ascents, you have here every shade of climate, from the growth of the sugar-cane, the indigenous and unequalled chirimoya, cotton, coffee, and vine, with the citron and orange, and the lemon-tree in endless blossom, and fruit in every stage of progress to maturity, to the maize, barley, wheat, and potato platforms in succession, till you reach the higher pastoral hill-tops, where it freezes after sunset for a great part of the year. But to return from this sweet vale of the brightest sun and softest moonlight, it remains to be noticed, that the higher elevations of the temperate section of the Andes, under 12,000 feet, are not in any very marked degree exempt from the pathological influences of the *Puna* range immediately above them. Yet every hundred yards of descent modifies and facilitates the cure of the severest inflammatory and hæmorrhagic diseases of the loftier platforms, until at the level of 10,000 feet and under, the most salubrious and invigorating climate in all Peru is reached. I have seen the mortiferous influenza of Cerro Pasco lose all its malignity as soon as it encountered these regions of medium temperature, where, indeed is enthroned the great Andine Sanitarium of both Coast and Cordillera. Here the hæmoptic, whether from the gelid heights or warm coast, finds his disease removed by the spontaneous evolution of nature; here, likewise, phthisis is never observed to originate, though the native Indians and Mestizoes of these much-favoured localities—alike removed from the extremes of cold or heat, dryness or moisture—are not exempt from pulmonary consumption when they take up their abode on the lower plains of the coast: And here again, the fair sons and daughters of the luxurious Lima seek and find a permanent cure from the earlier and well-defined symptoms of tubercular disease of the lungs. They leave off drugs, trust to climate alone, and thus verify the apothegm, " Levare dolorem tuam posset, si minus senare."*

The Typhoid Fever, or " Tarbardillo," of the Andes.—In Lima, when a fever degenerates from the intermittent or remittent to the continued form, and is attended with delirium, drowsiness, or stupor, a low and frequent, or small and fluttering pulse, with a dry, furred, and dark tongue, lips parched, and teeth with sordes, the patient is said to be "*atabardillado*," or affected with tabardillo. In the Sierra, again, at least on the higher as well as temperate ranges, the tabardillo is not intermittent or remittent in its origin, but from the beginning a continued fever. The phlegmasiæ of these regions never develop themselves in fevers of intermittent type, which seem to require the malarial element of warm and humid climates. Upon what anatomical lesions the difference of these fevers depend I shall not wait to inquire, nor can I, indeed, pretend to characterize the precise relations between these lesions and the special symptoms of each case. But just as on the coast intermittents are often distinguished, *symptomatically*, into terciana of the head, the eye, or stomach, so, also, in the Sierra, *tabardillo*, with gastro-enteric, or meningo-gastric symptoms, is called by the natives "*tabardillo-entripado*;" and pneumonia, complicated with tabardillo, is denominated "*Costado-entabardillado*." It is as complicated with phlegmasiæ of different organs that we practically have to do with this most fatal disease.*

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* In the "British and Foreign Medico-Chirurgical Review" for October 1856, No. xxxvi., will be found an article by me, in which, among other things, is pointed out in what stage or form of phthisis it is curable by change from the climate of the Coast to that of the Sierra; and the inland localities proved by long experience to be the best fitted to secure this important end.

The typhoid fever of the Sierra develops itself spontaneously, and often suddenly, but not contagiously, under its usual form of tabardillo. Indigestion and intemperance have much to do with this disorder, which usually follows on the endless saint-day festivals of the poor Indian. On these occasions he indulges in highly-spiced dishes, and drinks himself to forgetfulness, revels all night, perhaps in mud up to the ankles in wet weather, or exposed to the keen frosts of the dry season. When overcome by sleep, he lies down anywhere, and next day awakes chilled, and covered with snow, or soaked in rain; or, if in dry weather, half frozen by the night air, to be soon scorched by a burning sun; now, his head aches, his stomach is out of order, or his lungs are attacked, or he aches and chills with rheumatism all over; in short, he fevers, and is in for a tabardillo. This fever, at first attended with a strong and rapid pulse, soon passes to the lower type, indicated by small, frequent, or irregular pulse, delirium, coma, *subsultus tendinum*, with strongly marked pneumonic or gastric symptoms, which more peculiarly distinguish the course of the Sierra tabardillo from that ordinarily seen on the coast.

But while such may be considered the usual endemic fever of the Andine regions of Peru, in 1855 the temperate inter-Cordillera valleys had a dreadful epidemic, under the name of *Peste*, of which the published accounts in the Lima papers at different times showed the extraordinary mortality, especially in the department of Cuzco, where the Indian population is crowded, and large families are confined to small houses. This recent epidemic is described by one of my old colleagues in a letter to me, dated Lima, January 1856, as a contagious typhus, of which the cases that came under his notice were of the spotted kind, or typhus maculosus. He remarks, "In my opinion it resembles the Irish fever of the same name, on which Dr Graves has written so admirably well. Having first broken out in Ancash, it has crept on, extending itself little by little, until it has reached Cuzco and Puno, carrying destruction everywhere." It appears from official statements since published, that this typhus of the Sierra carried off its tens of thousands of the inhabitants ; while the yellow fever, by which it was preceded on the coast, probably did not cause so large a proportion of the whole mortality of 250,000, ascribed to the Peste,—on coasts and mountains,—from 1852 to 1856 :—for both the yellow fever on the coast, and typhus in the temperate Andine valleys, are considered to be the same disease, only modified by climate and elevation.

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* It is of the greatest practical importance to distinguish clearly between simple pleuro-pneumonia and pleurisy or pneumonia thus complicated with typhoid fever. The diagnosis must rule the treatment, which should be very different in forms of disease so essentially distinct in their nature and tendencies. On the typhoid epidemic fever of the Andes, I recently published a letter in the "Medical Times and Gazette," October 10, 1857.

This connection between yellow fever on the coast, and typhus maculosus on the Andine heights, is perhaps not so new as many at present believe. Don Antonio de Ulloa tells us that, in the year 1740, the yellow fever, or black vomit, first became known at Guayaquil. Aboard the South Sea galleons (said to have introduced the fever) at this time great numbers died of the pestilence—but fewer of the natives; and it does not appear then to have established itself firmly on the coast. But just as has now occurred in the Sierra of Peru, we find that in Quito there prevailed malignant spotted fevers and pleurisies, which swept away prodigious numbers. (See "Ulloa's Voyage to South America," vol. i., pp. 161, and 279.)*

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* I may here be permitted to refer to this excellent author, vol. i., p. 209, on a different subject, but intimately connected with the meteorology or the climate of the Andes. In speaking of his journey from Guayaquil to Quito, he remarks, "The canes are remarkable both for their length and thickness,

On the Climate and Diseases of the Montaña.—From the inter-Cordillera regions, the Montaña is entered at different breaks in the eastern Cordillera, as in the vicinity of Huanuco, Tarma, Huanta, &c.; so that we thus speak of the Montañas of each of these Sierra towns as if they were annexed to them. The nearest Montaña to Lima is that of Tarma. From the Pacific at Callao to the summit of the Cordillera at Antarangra the distance is 30 leagues; and from this Cordillera Pass to Fort San Ramon, lat. 11° 7′, on the River Chanchamayo, on the eastern slope of the Andes, it is, as nearly as may be, other 30 leagues. Thus the direct distance from the Pacific to the head of river navigation, on the eastern frontier of Peru, is 60 (Spanish) leagues, which maybe estimated at 200 English miles—for these leagues are long, and not over-carefully measured.* Fort San Ramon is situated at the foot of the eastern Cordillera, on the Atlantic slope, at the level of 2814 Spanish feet, and, following the windings of the rivers, at the distance of about 4000 miles from the mouth of the Amazon in the Atlantic Ocean. Dr Lorente of Lima, a Spaniard and able botanist, visited the Chanchamayo, and reported to Government on its climate, capabilities, and productions, which Report was published in the *Commercio* of Lima, 8th October 1853. He says, "here everything is on Nature's great scale. The whole country is

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and the water contained in their tubes. From the time of their first appearance till they attain their full perfection, when they are either cut down or of themselves begin to dry, most of their tubes contain a quantity of water; but, with this remarkable difference, that at full moon they are entirely, or very nearly, full; and with the decrease of the moon the water ebbs, till, at the conjunction, little or none is to be found. I have myself cut them at all seasons, so that I here advance nothing but what I know to be true from frequent experience. I have also observed that the water during its decrease appears turbid, but, about the time of the full moon, it is as clear as crystal." It were well that some qualified person on the spot would repeat and test these observations of Ulloa. They agree with the belief of the Peruvians on lunar influence, of which I took notice in my "Peru as it is" (vol. i., pp. 14, 15.), published by R. Bentley, London, 1839. In adverting to this work, let me say, that in the concluding chapter, titled, "*On Climate and Disease.—Panama, Guayaquil, Peru, and Chile,*" I have sketched a manual on these subjects, with prophylactic observations for the use of those who visit the regions to which they apply.

* The breadth of Peru from the Pacific to its eastern boundaries varies considerably at different latitudes. From Payta on the Pacific to the port of Tabatinga, on the river Amazon, the distance is estimated at 250 leagues.

one continuous forest, which, beginning at different heights, presents an undulating aspect. One moves on his way with trees before, above, and beneath him, in a deep abyss like the ocean. And in these woods, as on the immensity of the waters, the mind is bewildered; whatever way it directs the eye, there it meets the majesty of the infinite. The marvels of Nature are in these regions so common, that one becomes accustomed to behold without emotion trees whose tops exceed the height of 100 varas" (the *vara* is 2 feet 11 inches English), "with a proportionate thickness—beyond the belief of such as never saw them; and, supporting on their trunks a hundred different plants, they individually present rather the appearance of a small plantation than one great tree. It is only after you leave the woods, and ordinary objects of comparison present themselves to the mind, that you can realize in thought the colossal stature of these samples of Montaña vegetation." The same gentleman further adds,—"the climate is very healthy, though the temperature of the air sometimes rises to 28°"—I presume he means of Reaumur, which is the usual thermometer in Peru. I may here remark, that the bottom of the valley of the Chinchao, in the Montaña of Huanuco, where its river unites with the Huallaga, is about the same elevation and temperature as Fort San Ramon, at the junction of the Tulamayo with the river of Chanchamayo. In both these Montañas, the climate near the foot of the Andes is healthy, wherever the rivers do not, in the rainy season, overflow their banks; but where they do, malaria is generated. The district of Chanchamayo and Vitoc, however, is fortunate enough to have gentle slopes and lovely plains, over which the wind sweeps with freedom, thus ventilating and purifying the air, to invite the settlement of civilized man on this boundary-line of the naked savage of the forest.

Dr Lorente was the Professor of Botany in the College of St Fernando, when he was taken ill with hæmoptysis, and became unfit for his professional duties. He was long under medical treatment in Lima, and also tried the effects of the climate of Chorillos, the watering-place of that city, but became always weaker and more disabled. The result was, that his countryman Dr Pasaman, and myself, met in consultation, and arranged that he should go to Jauja, and thence to the less bracing but milder air of Huancayo: whence he visited the Montaña, when strong enough to do so. From the time he reached Juaja, the hæmoptysis ceased, as almost always happens with patients of this kind. It is therefore with peculiar emphasis he writes from the Chanchamayo, "The air is buoyant, and my lungs, which were oppressed in Lima and Chorillos, here perform their functions with the greatest ease." The Montaña, however, is for eight months in the year too humid for those affected with phthisis or hæmoptysis. From November to May it often rains for a week at a time. The dry season is very agreeable, but the ground, a short way below the surface, always preserves its moisture. The principal diseases that prevail along the swampy banks of the great rivers are dysentery and agues. Smallpox exterminated the population of Pozuzo on the Mayro, the route from Huanuco to Sarayacu on the Ucayli and great plains of Sacramento. At Sarayacu the maximum heat is registered at 85°, and the minimum at 74° Fahr.; the climate is delightful, and as described by the missionaries, upon the whole healthy. If we turn to the humid and sultry climate of the lower Mainas or Loreto, we there find that catarrh and rheumatism always attend the return of the wet season; as also fevers and agues, dysentery and diarrhœa, with their usual accompaniments of visceral obstructions and swellings. In the dry season, the sunstroke and erysipelas are familiar ills, and the *sarna* or *scabies* is inseparable from the condition of a semi-civilized population. Moyobamba, the capital of the province of Upper Mainas, contains about 5000 inhabitants of the Mestizo race, and, like Chachapoyas (the capital of the Amazonas department, lat. 6° 15′), enjoys a mild winter temperature of from 65° to 70° Fahr. in the shade. But in these parts plantains and other fruit are much used instead of bread, and therefore *vicho*, or dysentery, is endemic; while smallpox and other epidemics have committed, from time to time, great ravages. In the Lima newspaper, *El Comercio*, of the 11th October 1857, we read an official report of the Prefect of Moyobamba, Francisco Avarat de Ortez, intimating the rapid ravages of a fearful pestilence in that remote city of the Montaña. I here subjoin a translation of the symptoms of the disease, which the Government of the Republic is solemnly petitioned to use every possible means to avert, by sending to the aid of Moyobamba a salaried physician, and a suitable supply of medicines. The petitioners pathetically and forcibly plead their own patient endurance, without any physician among them, ever since the declaration of Peruvian Independence, though often scourged by epidemics; and that the fatal *Peste* which has just overrun nearly the whole length of the republic, must end in the total annihilation of its vastly reduced population, should the Government not exert itself on behalf of the people:—

"*Symptoms of the Epidemic which affects the City of Moyobamba and its Environs.*—An unnatural heat and shivering (calofrios); insufferable headache; fever, violent and continued; respiration hoarse, and attended with pain from the commencement of the attack; shooting pains in the scapulary region (pulmon); breath intolerable from the beginning. Throughout the course of the disease the patient feels great oppression (gran fatiga); pronounces his words in a broken tone, but shortly before death he becomes tranquil, and, speaking in the full possession of his reason, he expires. At the approach of death, the body begins to take on a dark hue, and very shortly after it, becomes entirely black and putrid. The duration of this disease does not exceed three days at most." This document is dated Moyobamba, September 9, 1857, and signed by Peter Bishop of Chachapoyas, and five other individuals. The symptoms of invasion, and the succeeding discoloration and calm before death, appear to be identical with the worst features of the Lima epidemic; which took on the form of typhus on crossing the first Cordillera, and now again having broken through the eastern barrier, assumes in the Montaña the intense type so graphically depicted by the Bishop and his coadjutors.

My limits compel me to conclude this rapid sketch of the geography of diseases in the climates of Peru. But I hope enough has been said by me here and elsewhere (see "Edin. Med. and Surg. Journal," vol. liii. to vol. lvii.), to show the different effects of climate, and especially the elevated and varied Andine climates, on diseases; and the important bearing of all the facts, collectively, on general physiology and pathology, as well as the more minute details of practical medicine.

On the Origin of the Muscular and of the Nerve Current in the Living or Recently Killed Animal:—A Polarized Condition of the Muscular and of the Nervous Tissue.

By H. F. Baxter.

In a former paper,[*] we were led to the conclusion that when a circuit was formed between the muscular or the nervous tissue and the venous blood flowing from the same part, that *current force* was manifested, and that this effect was due to the changes which occur *during* nutrition; thus confirming the inferences previously deduced by Matteucci, from his own experiments, as to the *origin* of the muscular current. Subsequent experiments have only tended to confirm those conclusions; and as no attempt has been made to support the objections then raised, or to refute the inferences then drawn, by experimental evidence, we cannot do better than refer to our original paper, convinced that a mere discussion as to matters of opinion will not alone assist in elucidating matters of fact. The following questions have, however, frequently occurred to us, Can the *whole* of the effect when the two surfaces alone—viz., the *transverse* or divided surface, and the *external* surface—of the muscle are formed into a circuit, be considered as *entirely* due to the changes which take place *during* nutrition? Where are the *anion* and *cation* of the circuit? If the electrode in contact with the divided surface be supposed to be in contact with the blood flowing from the divided bloodvessels, how is it that the electrode in contact with the external surface of the muscle should be *positive*, under the supposition that the effects are due to nutrition? Matteucci,[†] in some of his later researches, appears to entertain some difficulty in considering what is really the true electro-motor element of the circuit. To attempt to remove these doubts, and for the purpose of solving this question, the following investigation was undertaken.

For the convenience of discussion we may just sum up, in a few propositions, the results which appear to be well established

[*] Philosophical Magazine, Jan. 1856.

[†] Bibliothèque Universelle de Genève, Dec. 1856.

by the researches of Matteucci and Du Bois Reymond, and such as we have been enabled to confirm by our own experiments, but these we do not think it necessary to particularize.

1. *Any point of the surface of a muscle is positive in relation to any point of the divided or transverse section of the same muscle.*

2. *Any point of the surface of a nerve is positive in relation to any point of the divided or transverse section of the same nerve.*

These two propositions express the law as deduced by Du Bois Reymond, in regard to the *muscular* and the *nerve currents.*

Du Bois Reymond has, however, employed the terms *natural* and *artificial* to the different sections. There may be no objection to the employment of these terms; but unfortunately the so-called natural transverse section may act as the surface or longitudinal section in regard to the artificial section or the divided surface; and we do not think that the existence of the *para-electronomic* layer can be sufficiently made out to account for the difference of effect which may thus occur to justify us in employing these terms; nevertheless, we perfectly agree with Du Bois Reymond in considering that the *side* or *external surface*, or perhaps the sarcolemma of the muscle, and the *base* or *ends* of the muscular fibre, are the important points to be in contact with the electrodes for the production of the muscular current, and the similar parts in the nerve for the production of the *nerve* current.

3. The *intensity* of the *muscular* and *nerve* current depends upon the vital conditions of the tissues; the *current* does not subside immediately after the death of the animal, and evidently bears some relation to the state of its nutrition.

4. According to Matteucci,* the *intensity* of the muscular current varies as the *length* of the muscle in the circuit, and not according to the extent of its *transverse* section. This proposition is, however, rather difficult to prove; for, if two muscles of the same length, but of different thicknesses, be formed into a circuit, the thicker muscle will be found to give the more intense current (and the same will be found in re-

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* *Loc. cit.*

gard to the nerve), and we cannot help thinking that Du Bois Reymond is correct in concluding that the electro-motive force of muscles increases both with their length and thickness.

5. If the two ends—the divided or artificial transverse sections—of a muscle or of a nerve be placed between the electrodes of a galvanometer, no effect occurs upon the needle; but if one of the electrodes be placed upon the surface or longitudinal section, the other remaining in contact with the transverse section, the current is produced according to the law expressed in Proposition I.

6. If the electrodes be placed upon two symmetrical portions of the surface of the muscle or of the nerve, no effect is produced upon the needle.

The following experiments were now performed:—A muscle or nerve was divided into three or more portions, and then so arranged that the internal should form the outer portions, the continuity of the nerve being maintained. When the electrodes were placed at the two extremities, or upon symmetrical portions of the nerve or muscle, no effect, or, if any, but very slight indications were occasionally produced upon the needle; if the external surface and the divided surface were formed into a circuit, the current was obtained; and also when the portions were so arranged that the external surface of one portion was placed in contact with the transverse section of another, then the current was obtained, as in Matteucci's experiment, when forming a pile of muscular elements. These experiments, which we consider of some importance, were repeated several times upon the muscles and nerves of frogs, guinea pigs, and rabbits; care being taken to have the portions of nerve and muscle, as far as possible, of the same size and from the same nerve or muscle. They go far to show that the muscular or nerve fibre does not indicate any manifestation of *polarity* in regard to its *length;* there is no fact or evidence of any kind to show either that the nerve fibre or the muscular fibre presents a condition at one extremity opposed to that of the other extremity; there is no *antithetical* or *dual* condition made manifest indicative of *polarity;* whatever indication of *polarity* exists shows it to be manifested in the *transverse* direction, or rather that the *muscular fibre* or *nerve fibre* presents one condition, and perhaps the *sarcolemma* or the *neurilemma* the other; and we are now brought to the consideration of our original question, How far can this state be dependent upon the changes which occur *during* nutrition?

We must bear in mind, that when the *muscular tissue* or the *nerve tissue* are formed into a circuit with the *venous* blood flowing *from* the same part, we then get evidence of the manifestation of *current force*, the electrode in contact with the blood being *positive* to the other; the tissue may be thus considered as forming the *anion*, the *cation* existing in the blood as in ordinary secretion, the tissue being the secreted product. In the experiments we have now been considering, the electrode in contact with the sarcolemma or the neurilemma was *positive* to the other. Can we suppose the sarcolemma or the neurilemma to act as the *cation*, or as an *acid?* or might not the effect (the current) be due to the *electrical* condition of the muscular or nerve fibre, its *negatively* electric state being maintained so long as the vital conditions of the tissue continue?

Under the supposition that the effects might be due to the *heterogeneity* of the parts, the sarcolemma or the neurilemma acting as an *acid*, we now repeated the experiment, employing the following dilute solutions:—Five drops of strong sulphuric acid to one ounce of distilled water, formed the *acid* solution; one drachm of liq. potassæ (Phar. Londin.) to one ounce of distilled water formed the *alkaline* solution; and one drachm of common *salt* to one ounce of distilled water formed the *neutral* solution.

Whenever one electrode was moistened with the *acid* solution, this was *positive* to the other on whatever parts of the muscle or nerve they were placed. When *both* electrodes were moistened with the solution the indications were doubtful.

When the *alkaline* solution was employed in the same manner, the electrode in contact with the external surface of the muscle or nerve was *positive* to the other; it very rarely happened that the electrode in contact with the transverse section indicated a *positive* condition, when the other electrode alone was moistened with the solution.

With the solution of *salt,* similar effects were observed as with the *alkaline* solution; and from these facts we cannot but infer that these two solutions, the *alkaline* and the *salt,* acted merely as a conducting liquid, whilst the *acid* solution acted chemically upon the animal substances. We cannot suppose, or at any rate believe, that the sarcolemma or neurilemma acted, in these instances, as an *acid* substance, merely for the purpose of explaining or accounting for the existence and *direction* of the current, since the *alkaline* solution would have destroyed its *acid* reaction.

When the muscles and nerves had been placed in strong concentrated solutions, so as to act chemically upon the tissues, and different portions of them were then formed into circuits, the effects upon the needle were sometimes very powerful, sometimes null; the results, however, could never be predicated, and were evidently due to the chemical actions set up.

If the muscle or nerve was placed in hot water at the temperature of 180°, or in cold water at the temperature of 32°, so as to freeze it, the muscle or nerve current was seldom obtained.

If the muscle or nerve was squeezed up into a mass, so as to destroy its structure by mechanical means, no effect at all analogous to that of the muscular or nerve current was obtained; effects upon the needle were occasionally produced, but presenting quite a different character.

These results only tend to prove the conclusions already deduced by Matteucci and Du Bois Reymond, of the dependence of the *muscular* and *nerve* current upon the normal or *vital* condition of these two tissues, and go far to show that these currents cannot be *entirely* due to the *heterogeneity* of the parts in the circuit, but that whatever destroys the normal or healthy state of the tissues, destroys also the conditions upon which the muscular or nerve current depends.

If the existence of the muscular and nerve currents be dependent *entirely* upon the changes which occur during the *act* of nutrition, it is reasonable to suppose that the removal of the blood from the limb might perhaps prevent them from being manifested. We were now led to the following experiments:—

The animals—guinea-pigs, rabbits, and frogs—were bled to death, and the limbs and muscles then emptied of the blood, as far as possible, by squeezing the limbs; under these circumstances, however, the muscular and nerve currents were obtained. The amount of deflection was not perhaps so great as it would have been had the blood not been removed; nevertheless the current was manifested. We endeavoured to remove the blood by injecting water (at the temperature of 90° in the guinea-pigs and rabbits, and at the temperature of the atmosphere for the frogs) into the bloodvessels; and although the water escaped by the veins, we still obtained the current, amounting from 2° to 5° in the muscles, and from 1° to 2° in the nerves. The muscles in these latter experiments became turgid, but they were not so pale as we expected they would have become; and we very much doubt whether the blood was entirely removed from the limb or the muscular and the nerve tissue. These latter experiments, as far as they go, would tend to show the importance and dependence of the *current upon* nutrition; and although, in absence of further evidence, it would be impossible for us at present to state, or even conjecture, the period at which the act of nutrition terminates, there nevertheless appears to be a residual effect in these results which we cannot fully account for under the supposition that the *current* is due *entirely* to the changes which occur *during* nutrition, and we believe that we shall be justified in coming to the following conclusions, and in considering that the so-called muscular and nerve currents may depend upon three circumstances:—*first,* upon the changes which occur *during* nutrition; *secondly,* upon the *heterogeneity* of the parts (including under this term the action of the platinum electrodes, viz., the *catalytic* action or the *combining* power of platinum upon the moist animal substances in contact with its surface); and, *thirdly,* upon an *electrical* state or condition of the muscular or nerve fibre itself, which may be termed a *polarized* condition of the muscular or nerve fibre.

In thus considering the muscular or nerve fibre as being in a peculiar state or condition which we have termed *polar-*

*ized,** an objection might be urged to the employment of this term inasmuch as the term *polarity* embraces the idea of *duality*—an *antithetical* action, which, as we have seen, does not exist in regard to the fibre itself. We shall therefore endeavour to point out with what class of phenomena the facts appear to be the most nearly allied.

Nutrition, we believe, may be referred to the same class of actions as *secretion,* the tissue, muscular or nerve tissue, being deposited as a secreted product. These actions, *secretion,* we have already considered in our former papers as identical with those which occur in the *decomposing* cell of a voltaic circle, and to be POLAR† in their nature. The question, however, whether the *secreted* product and the *blood* can, when separated, maintain their peculiar electrical states we have not examined; but in regard to the *blood* we believe that that fluid *may,* from what we have occasionally observed in our former experiments;‡ at any rate, the improbability of the elements of compounds undergoing decomposition in a decomposing cell of a voltaic circle, retaining their peculiar electrical states would be no positive argument against this supposition; the state and condition of the liquids in animals, in regard to their fluidity, and the conditions under which the changes occur, present great differences compared to those that take place in ordinary voltaic decompositions; consequently, we may reasonably suppose that under the circumstances in which the changes occur in the animal body the electrical states of the solids and fluids may not be immediately lost. From these facts, we feel no difficulty or hesitation in regard to the *origin* of the muscular or of the nerve current in the experiments we have been

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* Dr Todd, we believe, has been the first to describe the true character of the phenomena we are now speaking of, and to point out their dependence upon nutrition, and also to consider them as being *polar* in their nature. We strongly recommend the perusal of the article on the Physiology of the Nervous System in the " Cyclopædia of Anatomy and Physiology."

† Phil. Trans., 1848, 1852; Phil. Mag., Jan. 1856; Edinburgh Phil. Jour. New Series, July 1856.

‡ In looking over, recently, some of the memoirs published at the time of the controversy between Galvani and Volta, we have met with a letter of Vassali Eandi, in which it will be seen that we have been completely anticipated in our investigations by this philosopher. It will be found in the Journal de Physique, t. xlviii. p. 336, 1799, Germinal an. vii., under the following title:—Lettre de VASSALI EANDI à J. C. DELAMETHRIE *Sur le galvanisme, et sur l'origine de l'électricité animale.* . . . . . . " si on compare," says Vassali Eandi, " le corps animal à la bouteille de Leyde lorsqu'on approche l'arc conducteur à la boule qui communique avec l'intérieur de la bouteille, tandis que l'autre extrémité de cet arc en touche la partie extérieure, on voit les corps légers s'élancer de la boule à l'arc; le même phénomène devrait avoir lieu dans la bouteille de Leyde animale, si je puis me servir de cette expression; cependant, quoique le D. Valli, le professeur Eandi et plusieurs autres ayent écrit qu'ils ont observé des mouvements électriques dans l'expérience de Galvani, comme il s'agit d'une expérience qui exige la plus grande délicatesse, ce que la moindre haleine agissant sur les corpuscules légers, peut tromper l'observateur, je vous dirai franchement que j'ai répété plusieurs fois cette expérience en changeant l'appareil, en faisant usage de feuilles d'or, et d'autres corps très-legers, et je n'ai jamais pu m'assurer qu'il

en résultât des mouvements électriques. . . . . Si j'avais une opinion à émettre, je serais portè à croire que les contractions musculaires sont produites par le mouvement de l'électricité animale dirigée par les conducteurs de l'électricité naturelle; car, sans alléguer en preuve de cette opinion les faits innombrables publiés par les D. Gardini, Bertholon, Cotugno, Galvani, Aldini, Valli, Eandi, Giulio, Rossi, Volta, &c., j'observerai seulement que dans la nature, chaque corps changeant son état chimique, change aussi sa capacité propre à contenir le fluide électrique, et même bien souvent il change de nature par rapport à l'électricité, comme on le voit dans les oxides métalliques. Or, puisqu'il n'y a aucun doute que l'air, dans la respiration, et les alimens, dans la digestion, ne changent d'état chimique, ils changeront donc aussi de capacité pour le fluide électrique. Réad a démontré que l'air dans la respiration, perd son électricité naturelle: j'ai prouvé ailleurs que les urines donnent une électricité négative, et j'ai fait voir plusieurs fois aux D. Gerri, Garetti, et aux élèves de médecine et de chirurgie, que le sang tiré des veines, donne dans mon appareil électromètrique, (décrit dans le vol. V^{e} *de l'Académie des Sciences de Turin*, Dec. 19, 1790) une électricité positive; donc l'électricité naturelle de l'air et des aliments reste dans certaines parties du corps en abondance, tandis que dans le même corps il y a d'autres parties qui, n'en ont pas la quantité proportionnée à leur capacité." We believe these and similar results have been attempted to be explained away, under the supposition that they *might* be due to chemical changes occurring *during* the evaporation of the fluids, or to the chemical reactions taking place between the fluids and the metallic vessels. There can be no doubt that the effects may be partly referred to these actions, but whether they can be *entirely* referred to them is very questionable. We strongly recommend the supporters of the opinion, that the *time* has not yet arrived for the prosecution of these inquiries, to peruse some of the memoirs published at the period we are now alluding to; they will find, if we are not much mistaken, that views which are now being considered as *novel* have already been developed, and what is more, supported by *experimental evidence* to a greater extent than is imagined. Our predecessors pursued and exhausted their subjects as far as they could, and then threw out conjectures for others to verify.

lately considering, viz., when the two surfaces of a muscle or of a nerve, the *transverse* section and the *external* surface are formed into a circuit. *During* nutrition, the tissue is deposited as an *anion*, the *cation* passing on in the blood; the *negative* electric condition of the tissue is maintained by nutrition, and continues in that state; it may, perhaps, be looked upon as being in a state of tension, *polarized;* and when the muscular or nerve fibre, and any other part—for instance, the sarcolemma or the neurilemma—are formed into a circuit, an effect is produced upon the needle, indicating the existence of a *current.* Under these conditions, the results become analogous or are identical with those that are observed with a charged Leyden jar, when the inner and outer surfaces are formed into a circuit; the tissue being *negatively* electrized may, perhaps, render the sarcolemma or neurilemma *positive* by *induction.**

In speaking, therefore, of the fibre as being in a *polarized* condition, it would lead to erroneous views if we supposed that one portion, taking its longest diameter, was *positive* or *negative* to the other, as a wire may be supposed to be when traversed by an electric current; the fibre would appear rather to represent an excited glass rod, as far as its mode of action may be considered, rendering other parts electric by *induction;* but whether the *force* exists of that *intensity* to produce attraction or repulsion, we have not been able to ascertain.† It does not necessarily follow that, because the *current* is produced, attraction and repulsion should also be obtained; the *intensity* requisite in the one case may be absent in the other; and we cannot do better than refer to the important paper, by Faraday, on the Gymnotus,* for the purpose of pointing out the great difference manifested in the two instances in regard to animal electricity and machine electricity. But the conditions under which the muscular or the nerve current may occur will be of the utmost importance to bear in mind: whether as the *act* of nutrition, or *during* nutrition; or whether as the *result* of nutrition, *i.e.*, from the *polarized* condition of the fibre itself, since they may be referred to two distinct class of actions. Their primary dependence, however, *upon* nutrition is a circumstance of some importance; and it would appear that this *polarized* condition of the tissue is intimately connected with the *vital* condition of the parts, and may be one mode by which *vitality* is manifested.

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* This *positive* condition of the external surface will go far to explain a circumstance which at one time rather perplexed us. When the *external* surface of a muscle, and the *venous* blood flowing from it, were formed into a circuit, the effect upon the needle was but slight; the external surface of the muscle and the blood being *both* positive, will fully account for the feebleness of the current then manifested.

† The following experiments may, perhaps, be of some interest. The limbs of frogs were suspended by means of a silk thread to a glass beam, which was also suspended horizontally by a silk thread attached to its centre, and fastened above to another glass rod. Upon holding a silk thread near to the limb, no attraction or repulsion of the thread was observed. Exciting a glass rod, and presenting it to the limb, the limb was powerfully attracted; it was attracted also upon presenting an excited stick of sealing-wax to it. But we never obtained any effect, when one limb was presented to the other, either of repulsion or attraction; it would appear as if one of the substances must be in an *excited* state to produce these phenomena. These experiments are not of much value,

As the constituents of a muscle or its particles must be in a state of self-repulsion, the electrized or polarized condition not being limited to the surface, as in a metallic conductor, but the whole substance of the muscle throughout being equally and bodily polarized, the question arises, Might not *muscular contraction* be the necessary result of the attraction between the particles, the muscular substance being, as it were, *depolarized* by *nervous* agency? and the *force* by which the state of tension in the muscle was maintained being evolved and set free or made manifest in some other *form* or mode af action, according to circumstances. In a former series of experiments,† we were led to the conclusion that, during *extraordinary* muscular exertion, some force *is* evolved, as in the fish; but during *ordinary* muscular exertion, the *force* may not become *free*, but be exerted in some other manner, as *heat;* or some of the organic actions, as *absorption* and *nutrition*, may be increased. But in these considerations we must not overlook the share the *nervous* system might have in the production

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and not at all adapted to eliminate any very correct result; but they tend to indicate the *negative* character of the tissue.

* Experimental Researches, vol. ii. p. 1.

† Phil. Mag., September 1855.

of some of these effects; and with our present knowledge on the subject, any further discussion upon it may be considered premature, and we shall therefore defer it.*

Before concluding, it may be remarked that we are now brought to the consideration of the explanation of Galvani's celebrated experiment, viz., the *contraction* that ensues when the nerve is brought into contact with the external surface of the muscle. Galvani compared the muscles to a charged Leyden jar, the two surfaces, the external and internal, being in opposite electric states; and he considered that when the circuit was completed, the *contraction* was a necessary consequence of the passage of electricity from one surface to the other by means of the nerve. No one can deny that Galvani was so far correct; he erred in considering the electricity as secreted in the brain, and transmitted by the nerves to different parts of the body, the muscles serving as mere reservoirs of the electricity; overlooking the fact, pardonable at his time, that electricity might be developed in other parts as well as in the brain. Volta erred in denying the *origin* of the power in the animal body, but was correct in pointing out that similar effects could be obtained by other means than with organic substances; the essential condition in all these experiments being, that the current should traverse the nerve, the *effect*—the muscular contraction—remaining the same whether the current had its origin in the animal body or from arrangements formed external to the body, and independent of it. Volta's experiments appear to have been adapted for the purpose of ascertaining the effect of *current* electricity upon the animal body; Galvani's for the purpose of ascertaining the *origin* of the effects that were observed to occur in the animal; and whatever views he might have entertained in regard to the nerve force being *identical* with ordinary electricity, all subsequent experiments only tend to show the great difference that exists between these two agents,—the action of current electricity, and that of nerve force. In saying this, however, we are very far from denying that *nerve force* is a *polar* force, and consequently it must bear some relation or *connection* with the other *polar* forces; *nerve force* being, as Dr Todd has pointed out, a higher *form* of polar force, and perhaps the *highest form* that we are acquainted with, and being such, we may reasonably suppose that its chief and perhaps peculiar *polar* characteristics can become manifested in the animal body alone.

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* Dr Radcliffe has published some views in regard to muscular contraction, in which he considers the muscle as being the seat of "*polar action*," and *contraction* as the result of molecular attraction. We cannot do better than refer to the paper for the arguments upon which his conclusions are founded, which will be found published in the *Medical Times and Gazette*, June 1855.

The following conclusions in regard to the *origin* of the so-called muscular and nerve currents may be deduced from the foregoing experiments:—

First, That they may depend upon the changes which occur *during* nutrition;

Secondly, They may depend, also, upon the *heterogeneity* of the parts (including under this term the action of the platinum electrodes, viz., the *catalytic* action or *combining* power of platinum upon the moist animal substances in contact with its surface); and also,

Thirdly, Upon a *polarized* condition of the nerve or muscular fibre;

Fourthly, That this *polarized* condition of the fibre is produced and maintained by *nutrition;* and,

Fifthly, That the circumstances under which the current may be produced may resemble, in one instance, those arising from the changes which occur in the decomposing cell of a voltaic circle, and in the other, those that arise from the action of a charged Leyden jar. In the latter case the current being due to the *polarized* condition of the fibre; in the former, to the *changes* which occur *during* nutrition.

On the Records of a Triassic Shore. By Professor HARKNESS, F.R.SS. L. & E., F.G.S.

The strata which in the south of Scotland represent the Permian formation, afford ample evidence of the prevalence of littoral conditions during the periods when these strata were being deposited. This evidence generally appears in the form of footprints, which impress the sandy deposits appertaining to this formation in several parts of Dumfriesshire; and besides footprints, we have also proofs of littoral conditions in the occurrence of the small pits caused by rain-drops on the surface of some of the arenaceous beds belonging to the same age in this district.

In strata of a newer period, in the same county, we find likewise features which inform us that littoral conditions have to a great extent obtained during the deposition of the beds which here seem to represent the Trias rather than the Permian age.

That large area of sandstone, which has its southern edge upon the "brockram" beds of Kirkby-Stephen and Brough, and which, extending northwards, flanks the western side of the Penine chain, where this chain separates Northumberland from Cumberland, and which, spreading itself northwestward over the more level portion of Cumberland, extends into Dumfriesshire, occupying the low country in the neighbourhood of Annan, affords evidence of the occurrence of littoral conditions to a greater extent than even the Permian formation.

There are two localities where we find the phenomena marking the prevalence of these conditions very well developed; one of these is in the course of the small river Shalk, about a mile south-east from the Curthwaite Station on the Carlisle and Maryport Railway. In this locality there is reason to believe that the sandstone was worked by the Romans; and here they in all probability obtained a portion of the materials used in the construction of the western extremity of the wall of Hadrian. The other locality which furnishes proofs of littoral conditions is at Corsehill Quarry, about two miles north of the town of Annan in Dumfriesshire. In both these localities the sandstone strata are in near proximity to the margin of the carboniferous area; and they appear to repose on breccias, such as those alluded to by Mr Binney, in his memoir on the Permians of the North of England, published in the Transactions of the Manchester Philosophical Society. These two localities present evidence of a like character, for on the surfaces of the sandstone beds the same features are exhibited both in Cumberland and in Dumfriesshire.

In their lithological nature the strata of these two districts bear great affinity to each other, consisting of red and yellow sandstones,—the latter not so abundant as the red-coloured strata; and with these are associated reddish purple clay-beds. These lithological features distinguish, where it makes its appearance, the Trias group from the Permian in the districts referred to; and the presence of numerous clay-beds serves well to mark the former from the latter; the Permians in the south of Scotland and in Cumberland rarely exhibiting any clay-strata accompanying the sandstone.

Among these littoral features of the Trias we find ripples which exist in a state of great perfection, and which appear to have originated from the action of a gentle wind blowing over shallow pools of water left by the retreating tide. In such a state of perfection are these ripples generally, that we rarely meet with anything on our present shores approaching them in beauty and regularity. Sometimes these ripple-marked sandstones manifest on their surfaces such an arrangement of ridges and furrows as must have resulted from a change in the direction of the current of wind which produced them. We have frequently presented to us a surface covered with ripples which cross each other at right angles; and yet the force of the current of wind which, in consequence of its change in direction, produced these crossing ripples was so slight as only partially to obliterate the ridges and furrows previously formed.

Besides the gentle ridge-and-furrow sand-ripples produced in shallow water, we have others of a less perfect nature. These are seen on the under surfaces of the sandstone strata which repose on the clay-beds. They consist of depressions and elevations, not arranged in a ridge-like form. They have been produced in deeper water on a muddy bottom; and we have natural casts of them presented by the under surfaces of the sandstones which repose on these clays.

There is another form of surface which we meet with on the under surfaces of the sandstones which rest upon the clay beds. This is in the form of markings, which are irregular in their occurrence, and which are confined to long stripe-like patches. In some instances the under surface of a sandstone stratum will be marked by those stripe-like patches for the width of a foot, covering a considerable length; and sometimes the width is greater, at others less. The surfaces of these markings are covered on the underside of the sandstone by numerous partially vermicular-like elevations, which bear a greater resemblance to the human thumb than any other figure. They are commonly in bold relief, and most frequently point in the same direction. Their position on the sandstones, and the relations which these bear to the clay-beds, show that these thumb-like elevations are casts derived from impressions on the clay strata. Their stripe-like arrangement, and the nature of the markings themselves, show that they owe their origin to the action of water; and there is strong reason for concluding that they have resulted from the action of little rills running over muddy patches on a stiff clay shore, which gradually wore shallow channels, and in their flowings produced the thumb-like ripples.

Besides these proofs of the action of water in motion, we find in some instances evidences of another character, which indicate the existence of other circumstances during the deposition of these sandstones. These occur in the form of elevations, which are of a track-like nature; and they have in general a shallow line traversing the centre of them. They differ in length, are less sinuous than ordinary worm-tracks, and have an average breadth of about the one-fifth of an inch. The animals which produced these tracks on the mud, and of which we have natural casts on the under-surfaces of the sandstones, seem rather to have been crustaceans than annelids; and on our present muddy shores, when the tide has left them uncovered, we have abundance of crustaceans of such a nature as to produce tracks of a similar character; crustaceans which drag themselves through the mud by means of a pair of strong legs in the anterior portion of the body, and by this means give rise to tracks bearing great resemblance to those which we find on the under-surfaces of the sandstones in a state of relief in these Triassic beds.

Besides these tracks, which seem to have resulted from crustaceans, we have the surfaces of the sandstones often marked by tracks of a more sinuous nature. These are often accompanied by small pit-like hollows, cavities which appear to have been the entrances into annelid burrows, and which have considerable affinity to the holes produced by our present lugworm.

The phenomena before alluded to are such as more or less result from the action or presence of water. Besides these, there are others which have emanated from the action of atmospheric causes. Desiccation cracks are abundantly manifested in the under-surfaces of the sandstones which repose on the clay-beds. These have resulted from the exposure of muddy patches to the influence of solar rays; and they have afterwards become filled up by the sand brought by the returning tide. We have them in the form of irregular ridges intersecting each other, and standing out in bold relief from the under-surfaces of the sandstone-beds; a mode of occurrence which usually obtains where desiccation cracks are seen.

In some cases, the under-surfaces of the sandstone strata present small pyramidal projections rising from the surfaces. On examination, the sides of these pyramids are seen to exhibit on their faces depressions of a hopper-shaped character. These little projections are the casts of hollows occurring in the clay-beds, the fertile source of the numerous markings which cover the under-surfaces of the sandy deposits. These pyramidal projections are pseudomorphs, which have originated from crystals of bay-salt originally formed in little pools on the shore during the recession of the tide. Evaporation of sea-water having taken place, crystals of sea-salt were produced, having their solid angles partially sunk in the mud; and the returning tide bringing with it fine sand, dissolved these crystals, depositing the fine sand in the hollows produced in the mud by the solid angles, gave rise to the occurrence of the pyramidal elevations which are sometimes found covering the lower surfaces of the sandstones.

The influence of atmospheric causes is also manifested to us by another circumstance. This is the occurrence of small pitted hollows on the upper sides of some of the sandstones; and on the lower side of the succeeding stratum natural casts of these hollows are seen in relief. These hollows are the effect of rain-drops, and in general they are regularly rounded in their form, having resulted from rain falling nearly perpendicularly. In some cases, this regular rounded form does not occur. In its place we have very oblong impressions and casts, one extremity of the oblong being deeper in the impression and higher on the cast than the other. Here we have proofs of the effects of a drifting rain carried by a strong wind; and those oblong impressions being generally of greater size than the round ones, point out the larger size of the raindrops which produced them.

On these ancient shores we find proofs of physical conditions of a very perfect character, and evidence of like conditions may be met with on our present shores, resulting from the same causes, where we have the occurrence of such circumstances as favour the production and preservation of the phenomena which emanate from these conditions.

Observations on the Temperature of the Pentland Frith, made by J. J. Cochrane, *during Consecutive Tides,* for T. Stevenson, F.R.S.E., Civil Engineer.

The Scotch Meteorological Society have, among other interesting investigations, resolved to prosecute that suggested by our late distinguished naturalist Dr Fleming, for ascertaining the temperature of the sea at various points of the coast, so as to discover in what way the temperature of its waters affected the climate of Scotland, and whether the effects of the Gulf-stream are really discernible on our shores, as has frequently been alleged. The Society has now many observers in this field, several of whom are superintendents of lighthouses, harbours, and other public works with which I am professionally connected, who make periodical returns to Dr Stark, the Secretary of the Society. Among these the island of Stroma seemed to offer peculiar facilities, as being situated in the middle of the Pentland Frith, through whose narrow channel so large a portion of the tidal water passes. Besides the regular observations which I requested Mr Cochrane (who has charge of the erection of a beacon at Stroma) to make for the Society, I proposed to him to observe the temperature of the sea at every half hour during two consecutive tides. In this way it seemed to me probable that, by the observations of *a single day*, a decisive result might be at once obtained. I beg to communicate Mr Cochrane's letter, with the results of his observations, from which it will be observed that there did apparently exist a trifling difference of temperature ($\frac{1}{8}$°) between the flood and ebb tide of the day (15th June) on which the observations were made; that the temperature from 10 feet to 50 feet below the surface remained constant, and that while the temperature of the air rose 7½°, that of the sea hardly seems to have been affected by that change, as the temperature at 10 feet still remained the same as that at 50 feet, which could hardly have been the case had the rise ($\frac{1}{8}$°) been due to any superficial change. I may add, that the thermometer used was of the form approved by the Meteorological Society, there being a cistern attached for bringing up water from any depth to which the instrument is plunged below the surface. By this arrangement there is no risk of the thermometer being affected by changes of temperature in the surrounding media, during the short time that is required for lifting it up, and for reading off the result.

"*Stroma*, 15*th June* 1857.

"DEAR SIR,—I received your esteemed favour of the 8th instant on Friday evening, and regret that the weather and other causes have prevented me taking any continuous observations, as I have never had an entire day I could devote to that purpose; but to-day, owing to the calmness of the weather and lowness of the neap tides, I have been able to make some satisfactory ones, although, perhaps, not productive of the result expected.

"I intended to have taken them every half hour, but found it required all that time to take them properly, and nearly as much to regain the position from which we had been carried by the tide. The ebb-tide ran in the centre of the Frith till about half-past eleven or a quarter to twelve o'clock, so that I was able to make observations during two entire tides.

"During all the day the wind blew with variations from the southward; in the morning it was foggy and dull; about 10 A.M. the wind increased a little, the fog began to clear, with occasional sunshine, and from 2 P.M. the evening was clear, with sunshine.

"During all this time, while the air varied 7½°, the water did not vary above ⅛°; for the observations marked 48¾ were full, while those marked 49 were scarcely up to the mark, and I could not perceive the slightest difference in the temperature of the water at 10 and at 50 feet.—I am, &c.

(Signed) "JOHN J. COCHRANE.

"T. Stevenson, Esq., Civil Engineer, Edinburgh."

Thermometrical Observations taken in the Pentland Frith between Caithness and the Island of Stroma, 15th June 1857.

Tide.	Time.	Air.	Depth of Water at which Temperature was taken.				
			10 Feet.	20 Feet.	30 Feet.	40 Feet.	50 Feet.
Ebb.	6 A.M.	49¾°	48¾°	48¾°	48¾°	48¾°	48¾°
,,	7 A.M.	50	48¾	48¾	48¾	48¾	48¾
,,	8 A.M.	51	48¾	48¾	48¾	48¾	48¾
,,	9 A.M.	52¼	48¾	48¾	48¾	48¾	48¾
,,	10 A.M.	52	48¾	48¾	48¾	48¾	48
,,	11 A.M.	54½	48¾	48¾	48¾	48¾	48¾
Flood.	12	57¼	49	49	49	49	49
,,	1 P.M.	54	48¾	48¾	48¾	48¾	48¾
,,	2 P.M.	56¼	49	49	49	49	49
,,	3 P.M.	52¾	49	49	49	49	49
,,	4 P.M.	52½	49	49	49	49	49
,,	5 P.M.	52	48¾	48¾	48¾	48¾	48¾
Ebb.	6 P.M.	50¾	48¾	48¾	48¾	48¾	48¾

I have, since the above was written, received from Mr Cochrane the following letter and observations, which seem rather to disprove the existence of any excess of temperature of the flood over the ebb tide:—

"*Stroma Beacon Works*, 15*th Aug.* 1857.

"Dear Sir,—The observations were made at ¾ ebb and ¼ flood, and the line was allowed to run out to the length of fifty feet, but from the rush of tide not more than twenty feet can, I think, be depended upon.

"As, in the former ones, the difference of temperature in no case exceeded ⅛°, while the air varied 15¾°; and on two occasions the water during ebb seems, if anything, to have been higher than during flood.

"I regret I had not got two thermometers, as I could have taken the temperature of the air more accurately had two been given me.—I am, &c.

(Signed) "John J. Cochrane.

"T. Stevenson, Esq., Civil Engineer."

Thermometrical Observations taken in the Pentland Frith during Flood and Ebb-Tides.

Date.	Air.	Ebb Tide.	Air.	Flood Tide.
1857.				
July 4.	58¾°	51°	55°	51°
„ 10.	54½	51¼	53¾	51⅛
„ 13.	61	51¼	63	51¼
„ 15.	66	51⅛	67	51¼
„ 18.	65	51¾	59½	51¾
„ 21.	60½	51¾	60¼	51¾
„ 25.	52	51¾	53¼	51¾

I have only to regret that, the beacon at Stroma being long since finished, I have now no opportunity of continuing the observations on the temperature of the Pentland Frith during the winter months. The results which have been obtained show at least how materially the climate must be affected by our insular position. The waters which surround us are proved to be great reservoirs of heat, as indeed we had every reason to believe was the case.

17 Heriot Row, *Nov.* 25, 1857.

On the Composition of the Building Sandstones of Craigleith, Binnie, Gifnock, and Partick Bridge. By THOMAS BLOXAM, Assistant Chemist, Laboratory of Industrial Museum. *With a Preliminary Note* by Professor GEORGE WILSON, Director of the Industrial Museum.*

PRELIMINARY NOTE.

In prosecution of the analyses of Scottish building stones commenced last winter in the laboratory of the Industrial Museum, by the examination of the bed-rock from Craigleith quarry,† four more sandstones have been analyzed since May 1856 by Mr Bloxam. The stones in question are the Craigleith liver-rock, and the Binnie sandstone, from the neighbourhood of Edinburgh; and the Gifnock and Partick Bridge stones, from the neighbourhood of Glasgow.

As in the case of the coarser Craigleith rock, the chief points inquired into, in the case of each stone, have been the following:—

1. The specific gravity.
2. The amount of water naturally present.
3. The amount of water absorbed by entire aqueous immersion under air.
4. The amount of water absorbed by partial aqueous immersion, distinguished in the sequel as absorption by "capillary attraction."
5. The amount of water absorbed by entire aqueous immersion under the air-pump vacuum.
6. The amount of substance soluble in pure water.
7. The amount of substance soluble in water saturated with carbonic acid.
8. The amount of substance soluble in dilute hydrochloric acid.
9. The amount of clay present.
10. The quantitative composition.

From the entire investigation it will be seen that, as in the case of the Craigleith bed-rock, water alone dissolves something from each stone; water charged with carbonic acid, dissolves an additional amount of substance; and water containing mineral acids, effects still further solution. The conviction I had long entertained, that the iron-stains in sandstones are occasioned not only by the oxidation of iron pyrites, but by the solution of iron in water containing carbonic acid, and which led to the trial in the case of the Craigleith bed-rock of the action of carbonic acid water upon its powder, is now extended and confirmed.

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* Communicated to the Royal Society, 21st December 1857.

† See Proceedings of the Royal Society of Edinburgh, vol. iii. p. 390.

The results in full are stated in the succeeding statements by Mr Bloxam, who has had the entire charge of the analytical inquiry. His interesting observation, that cobalt occurs in Craigleith stone, previously announced in relation to its coarser variety, is now extended to the denser liver-rock and to the Gifnock sandstone.

Copper also has been shown to be present in the Binnie sandstone, a metal not hitherto suspected to exist in rocks of its class. Mr Bloxam has also pointed out the occurrence of nodules of protocarbonate of iron in the Partick stone, a peculiarity which probably will not be found confined to that rock; since, in truth, it is but the most exaggerated form of that occurrence of carbonate of iron in sandstones, to which the extraction of iron from them by carbonic acid water pointed. Nevertheless, I was quite unprepared for the carbonate of iron occurring in separate masses of considerable magnitude, nor was it in consequence of any hypothesis, but solely by careful analysis, that Mr Bloxam made this curious discovery. The explicit table which he has constructed, and the commentary which precedes it, render any further remarks on my part unnecessary. G. W.

Details of Analysis.

1. *Craigleith Liver Sandstone.*

1. The specific gravity, taken as a mean of three experiments, gave a result of 2·432. A calculation afforded by this experiment gave the weight of a cubic foot as 151·43 lbs.
2. The water it contained in its natural state was 3·2 ounces per cubic foot, or ·13 per cent.
3. The amount of water absorbed by this stone was found to be 5 imp. pints per cubic foot.
4. The amount of water it was capable of absorbing by capillary attraction, was 5·11 imp. pints per cubic foot.
5. When placed under the exhausted receiver, and in a vessel of water, it was found to absorb 7·7 imp. pints per cubic foot.
6. The action of water, tried in the usual manner, dissolved ·38 of residue from 400 grains of stone. It was analyzed, and found to contain silica, iron, lime, magnesia, and soda.
7. The action of carbonic acid gas, when passed through water in which the pulverized stone was in suspension, was to dissolve protoxide of iron, lime, and soda.

8. Hydrochloric acid allowed to act upon the stone dissolved silica, alumina, iron, cobalt, lime, magnesia, and soda.

A special examination for cobalt was made upon a large quantity of the stone, when it was found in quantity quite sufficient for detection by the usual reactions.

9. The clay ascertained by the modification of Mr Napier's process* amounted to 4 per cent.
10. The quantitative composition of the stone was ascertained to be:

Silica, ...	96·99
Iron and alumina, . . .	2·95
Water, ...	·13
	100·07

2. *Binnie Sandstone.*

The second stone subjected to analysis was procured from Binnie quarry; it was chosen not only as a building material in great repute, but also with the view of investigating the bituminous matter which is both disseminated through it, and found in sufficiently large quantities to admit of a special inquiry.

In the specimen alluded to, the bitumen appeared in small spots, becoming more visible when the stone was heated to 212° Fahr. When held in a flame, it melted, burned, and left the stone quite white.

To the consideration of this curious substance the last part of this paper is entirely devoted; and the experiments upon the stone itself are here resumed, in order to complete this part of the inquiry.

1. The specific gravity of Binnie stone is 2·413, and the weight of a cubic foot 150·19 lbs.
2. It contains in its natural state 5·5 oz., or ·23 per cent. of water.
3. A cubic foot of the stone absorbed 6·1 imp. pints, when simply immersed in water.
4. By capillary attraction it gained 5·5 imp. pints per cubic foot.
5. Placed in water and under the air-pump receiver, it gained 7·85 imp. pints per cubic foot.
6. Water dissolved from 400 grs. of the stone ·23 of a grain.
7. Carbonic acid was found to dissolve magnesia, lime, soda, and a trace of iron.
8. Hydrochloric acid dissolved iron, lime, magnesia, a small quantity of potash and soda, and a substance hitherto unmet with in sandstones, namely, copper.

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* Proceedings of the Royal Society of Edinburgh, vol. iii. p. 391.

The indications of the presence of copper in this solution, led to a special examination for that substance, when it was found to exist without doubt.

9. The amount of clay in Binnie stone was 1·7 per cent.
10. The quantitative composition is—

Silica, ...	92·66
Peroxide of iron and alumina, . .	4·88
Organic matter, ...	2·23
Water, ...	·23
	100·00

I now proceed to a few remarks upon the bituminous substance already briefly noticed, as occurring in the Binnie stone.

It is a brittle substance, resembling wax to the touch, fusing at 240° Fahr., and boiling above 680° Fahr.

It is slightly soluble in alcohol, imparting to it an acid reaction; it is somewhat more soluble in ether, in which case the solution also has an acid reaction. It is also soluble to a slight extent in bisulphide of carbon; turpentine, however, is its best solvent, giving a solution of a brown colour.

The specific gravity of the bitumen is ·955; when heated it completely melts, then boils, and finally burns away, leaving a trace of ash.

A large quantity of it was burned and the ash examined, when the following substances were found:—silica, iron, soda, and magnesia.

When subjected to destructive distillation, it furnishes two different products: the first solidifies as soon as it distils over; the second remains liquid even at 32° Fahr.; and exhibits the properties of paraffine oil.

The first product, when treated with ether, yielded paraffine in large quantity.

A quantitative estimation of the ash and volatile matter gave the following results:—

Volatile, ...	99·86
Ash, ...	·06
	99·92

Water ·68 per cent.

A portion of the bitumen was subjected to organic analysis, with chromate of lead, and gave a mean result as follows:—

Carbon,	84·37
Hydrogen,	14·89
Water at 212° Fahr.	00·68
Inorganic constituents,	·06
	100·00

3. *Gifnock Sandstone.*

The third variety submitted to investigation was from Gifnock Quarry, situated between 2 or 3 miles north of Glasgow; it was procured from Mr Napier, chemist, Partick, Glasgow.

The stone appeared much disintegrated and easily broken.

1. The specific gravity was ascertained to be 2·463, and the weight of a cubic foot of the stone, 153·49 lbs.
2. In its natural state, this stone contains 1·3 oz. per cubic foot, or ·05 per cent.
3. The amount of water absorbed by the stone, was 6·7 imp. pints per cubic foot.
4. By capillary attraction, it absorbed 7·4 imp. pints per cubic foot.
5. When placed under the air-pump receiver, and in a glass of water, it was found to absorb 8·9 imp. pints per cubic foot.
6. The action of water on 400 grs. of the stone was to dissolve ·26 of a grain.
7. Carbonic acid gas was found to dissolve protoxide of iron, lime, alumina, and magnesia. No doubt the protoxide of iron and the lime existed as carbonates.
8. Hydrochloric acid was found to dissolve from the stone, silica, protoxide and peroxide of iron, much lime, magnesia, soda, manganese a trace; and cobalt in larger quantity than from any of the former stones.

Upon the addition of hydrochloric acid to this stone a most abundant escape of carbonic acid took place, partly due to the proto-carbonate of iron (as will be seen by a subsequent experiment) and carbonate of lime present.

9. The clay in the stone was found to be 11·6 per cent.
10. The quantitative composition of Gifnock stone is as follows:—

Silica,	85·55
Carbonate of lime,	7·90
Peroxide of iron and alumina,	6·55
Water,	·05
	100·05

Much of the iron existed as peroxide, due to exposure to the atmosphere.

4. *Partick Bridge Sandstone.*

The fourth stone made the subject of experiment was Partick Bridge Quarry, about a mile and a half due west from Glasgow.

In the preliminary process of pulverization preparatory to analysis, some pieces of a black coloured substance, associated with iron pyrites, were found disseminated through the stone, which were carefully separated, and made the subject of special inquiry.

When heated, this substance blackened, due to the presence of a small quantity of organic matter; its solubility in different menstrua was ascertained, dilute hydrochloric acid being first added; it had, however, little or no action.

The probability of this substance being clay was suggested to me by others; but from its extreme hardness and general weight the supposition did not seem likely. I was led, therefore, to try it by fusion with alkaline carbonates. The fused mass was treated, as usual, with dilute hydrochloric acid, when a black residue was left, which entirely dissolved in more concentrated acid.

A small portion of this powder was collected and examined; it was attracted by the magnet, and its solution in hydrochloric acid yielded nothing but iron in the state of protoxide. This circumstance suggested the probability of the supposed clay being, firstly, clay very rich in protoxide of iron; or secondly, entirely an iron compound, devoid, or nearly so, of clay; for, on examining the acid solution of the fused mass, nothing but a trace of alumina was discovered, at once proving the absence of all clay.

The black powder attracted by the magnet yielded by analysis 5·78 peroxide of iron, from 6·01 of substance; while, had the substance been magnetic oxide of iron, the amount of peroxide yielded would have been 6·63; so that we may safely conclude that the substance was nothing more than magnetic oxide of iron, produced during the fusion with the alkaline carbonate.

The next experiment that suggested itself was to try the action of more concentrated acid upon the supposed clay. The whole of the substance immediately dissolved in moderately strong hydrochloric acid, with the evolution of much carbonic acid gas; the solution on analysis yielded a large quantity of protoxide of iron, together with carbonate of lime, and traces of sulphate of lime, also alumina, magnesia, and soda.

As a conclusive experiment, the action of carbonic acid gas upon the substance suspended in water was tried. Upon filtering the liquid at the close of the experiment, and subsequently analyzing it, much protoxide of iron was dissolved.

It is obvious, from the foregoing remarks, that the pieces of substance found disseminated through the stone consist entirely of proto-carbonate of iron.

The ill effects of these nodules of proto-carbonate of iron are at once evident; for a block of stone freshly cut from the quarry exhibits no external mark of their presence within it to guide us, and it is not until the rain and air have had their full effect upon it for some time that the stain renders itself visible as a dark reddish-brown ring of peroxide of iron.

1. The mean specific gravity of the Partick stone was 2·503, and the weight of a cubic foot 156·42 lbs.
2. In its ordinary condition the stone contained 2·2 oz. or ·089 per cent. of water.
3. The amount of water absorbed was 7·05 imp. pints per cubic foot.
4. Its power of absorbing by capillary attraction was 5·11 imp. pints per cubic foot.
5. In the exhausted receiver of the air-pump the stone absorbed 7·7 imp. pints per cubic foot.
6. Water dissolved from 400 grains of the stone ·11 of a grain, and the solution contained iron, lime, magnesia, with traces of alkalies.
7. Carbonic acid dissolved lime, magnesia, iron, and soda.
8. Hydrochloric acid dissolved the following substances:—Protoxide of iron, and a little peroxide, lime, magnesia, alumina, silica, and soda, in small quantities.

When hydrochloric acid was added to the stone, a decided smell of sulphuretted hydrogen was observed, and the gas was also detected by paper moistened with acetate of lead; it was due to the sulphuret of iron before noticed.

9. The clay in the stone was found to be 29·5 per cent.
10. The quantitative composition is thus expressed—

Silica	84·85
Iron and alumina . . .	9·35
Carbonate of lime, . . .	4·65
Magnesia, ...	·45
Alkalies, . . .	·62
Water, ...	·08
	100·00

The entire results are given in the succeeding Table:—

Analysis of Building Sandstones.

Name of Stone.	Specific Gravity.	Water naturally contained, or Loss on Drying, at 212° F.	Water absorbed by continued immersion.	Water absorbed by Capillary Attraction.	Water absorbed under the Air-Pump.	Action of Water on the Stone.	Action of Carbonic Acid Water upon the Stone.	Action of dilute Hydrochloric Acid on the Stone.
Craigleith Liver-Rock.	2·432.	3·2 fluid ounces per cubic foot.	5· imperial pints per cubic foot.	5·11 imperial pints per cubic foot.	7·7 imperial pints per cubic foot.	6 ounces boiled on 400 grains for 1 hour 40 minutes, dissolved from it ·38 of a grain.	Dissolved Protoxide of Iron, Lime, and Soda.	Dissolved Silica, Alumina, Iron, Cobalt, Lime, Magnesia, and Soda.
Craigleith Common Rock.	2·443.	5·7 fluid ounces per cubic foot.	3·8 imperial pints per cubic foot.	4·2 imperial pints per cubic foot.	6·2 imperial pints per cubic foot.	6 ounces boiled on 400 grains of stone for 1 hour 40 minutes, dissolved from it ·35 of a grain.	Dissolved Protoxide of Iron, Lime, and Magnesia.	Dissolved Protoxide of Iron, Peroxide of Iron, Oxide of Cobalt, Manganese a trace, Alumina, Lime, Potassa, and Soda, in small quantity.
Binnie Stone.	2·413.	5·5 fluid ounces per cubic foot.	6·1 imperial pints per cubic foot.	5·5 imperial pints per cubic foot.	7·85 imperial pints per cubic foot.	6 ounces boiled on 400 grains, dissolved ·23 of a grain.	Dissolved Magnesia, Lime, Soda, with a trace of Iron.	Dissolved Iron, Lime, Magnesia, Potassa, and Soda, and Copper.
Gifnock Stone.	2·463.	1·3 fluid ounces per cubic foot.	6·7 imperial pints per cubic foot.	7·4 imperial pints per cubic foot.	8·9 imperial pints per cubic foot.	6 ounces boiled on 400 grains, dissolved ·26 of a grain.	Dissolved Protoxide of Iron, Lime, Alumina, and Magnesia.	Dissolved Silica, Protoxide and Peroxide of Iron, much Lime, Magnesia, Soda, a trace of Manganese, and Cobalt in larger quantity than any of the former.
Partick Bridge Quarry.	2·503.	2·2 fluid ounces per cubic foot.	7·05 imperial pints per cubic foot.	5·11 imperial pints per cubic foot.	7·7 imperial pints per cubic foot.	6 ounces boiled on 400 grains, dissolved ·11 of a grain.	Dissolved Protoxide of Iron, Lime, Magnesia, and Soda.	Dissolved Protoxide of Iron, Peroxide of Iron, small quantity; Lime, Magnesia, Alumina, Silica, and Soda.

COMPOSITION IN 100 PARTS.

	Craigleith Liver Rock.	Craigleith Common Rock.		Binnie Stone.		Gifnock Stone.		Partick Bridge Quarry.
Silica,	96·99	96·95	...	92·66	...	85·55	...	84·85
Peroxide of Iron and Alumina,	2·95	2·30	...	4·88	...	6·55	...	9·35
Water,	·13	·23	Organic Matter,	2·23	Carbonate of Lime,	7·90	Carbonate of Lime,	4·65
Lime and Magnesia, Oxide of Cobalt, and Alkalies,	00·00	·52	Water,	·23	...		Magnesia,	·45
					Water,	·05	Water,	·08
							Alkalies,	·62
	100·07	100·00		100·00		100·05		100·00

The Rotatory Theories of Storms. By R. RUSSELL, Kilwhiss, Fife.

"Making certain authors dictators instead of consuls, is the principal cause that the sciences are no farther advanced. Learners owe to their masters only a temporary belief, and a suspension of their own judgment till they are fully instructed, and not an absolute resignation and perpetual captivity. Let great authors therefore have their due, but not so as to defraud time, which is the author of authors, and the parent of truth."—*Lord Bacon.*

The object of this paper is to draw the attention of the readers of this Journal to the anomalous condition of that branch of meteorological science which relates to the action of winds during storms. On this question meteorologists are chiefly divided into two classes—one holding that storms are usually vast portions of the atmosphere in a state of rotation—the other that there is no evidence of rotation.

It is not a little singular that opinions of so opposite a nature should exist upon a subject which can be so readily tested by observation. If there be no rotation of winds during storms, it must one day be a matter of historical interest to look back upon the nature of the evidence which has been adduced in support of the rotatory theory, and which has led so many to adopt it.

We may premise, however, that the general term rotatory theory is so far a misnomer; for there are several rotatory theories—five at least—all differing very widely from each other. That so many different theories have been put forth by thinking men in their endeavours to explain the phenomena of storms on the principle of rotation, indicates the difficulties of the subject. It is likely that those who have not examined the question carefully will be somewhat surprised and amused at the curious variety of opinions comprehended under the title of the Rotatory Theory.

Impressed with the conviction that the phenomena of winds and storms cannot be accounted for by any of the rotatory theories which have been put forth, we shall proceed to state the physical objections to the rotatory theories of Reid, Redfield, Dove, Thom, and Herschel. Then we shall point out the erroneous method of observation which has led to the supposition that the wind in storms moves in circles. In another paper we shall explain the difference in our mode of accounting for the action of winds from that of Professor Espy.

Of all the theories which have been proposed to explain the phenomena observed during storms, or even during our ordinary weather, no one at first sight appears more simple than that which has been advocated by Sir William Reid. According to this authority, the veerings of the wind, the changes of temperature, the precipitation of moisture, the fall and rise of the barometer, can be readily explained on the supposition that storms are vast bodies of air in a state of rotation and translation; in short, vast whirlwinds moving over portions of the surface of the globe. The following extract gives a very lucid exposition of Sir William Reid's rotatory theory :—

"The discovery that great storms are progressive whirlwinds, led Mr Redfield to the explanation of what I believe to be the true cause of the fall and rise of the barometer in gales of wind. His explanation is, that a whirlwind which sets an extended portion of the atmosphere into a state of rapid revolution, diminishes the pressure of the atmosphere over that portion of the earth's surface, and most of all at the centre of the whirl.

"This idea may be exemplified by taking a tumbler half full of water, and after putting the water in rapid revolution, holding it up against a strong light, the surface of the water will be depressed in the centre of the whirl. The liquid will serve to represent the atmosphere; and if the tumbler be moved over a fixed point in the manner in which a progressive whirlwind gale would move over it, it will show how the barometer begins to fall as the storm sets in, how it continues to fall until the centre has passed, and afterwards rises and resumes its former level."

With Sir William Reid all other rotatory theorists have been led to the conclusion that these so-called vast whirlwinds gyrate in the Northern Hemisphere from right to left, or in a direction contrary to the movement of the hands of a watch, while in the Southern Hemisphere may move from left to right.

In the meantime we may here point out the difference betwixt the rotatory theory of Sir William Reid and the rotatory theory of Mr Redfield. The former, in his endeavours to account for all the phenomena, supposes that the air in the centre of a revolving storm, like the water whirling in a tumbler, *descends*; the latter, that the air in storms moves spirally inwards, and *ascends* in the centre. Thus, upon this point, these two authorities hold quite opposite opinions.

> "Great whirlwinds, by lowering the upper atmosphere, bring down portions of the colder regions of the air, and these, mingling with the warmer and moister air at the surface of the sea, form very dense clouds."—*Reid.*

> "We may expect to find, in the path of the whirlwind, strong evidence of the inward or vorticular course of the wind at the earth's surface; the violence of which inward motion is clearly indicated by the force with which various objects, often of much weight, are carried *spirally* upward from the axis of the revolving body."—*Redfield on the New Brunswick Tornado.*

Not only does Mr Redfield consider that the winds in the famous New Brunswick tornado blow *spirally inwards and upwards*, but he applies the same principle to more extended storms, such as in the case of the Cuba hurricane, when he writes—"The involution seems to afford a measure of *the air and vapour* which finds its way to a *higher elevation* by means of the vortical movement in the body of the storm."

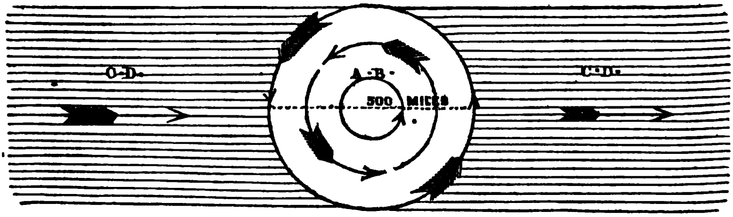
Mr Redfield, it would appear, is now quite aware that Sir William Reid's theory of the rain being produced by a *descent* of air in the centre of storms is altogether untenable, seeing it is the ascent of air saturated with the vapour of water to a higher elevation, which is one of the chief causes of precipitation. To escape from a number of phenomena which indicate an inblowing of the winds during storms, Mr Redfield now maintains that the motion is spiral instead of direct, as is advocated by Professor Espy.

Both Mr Thom and Professor Taylor advocate a rotatory theory, wherein the winds gyrate, and at the same time move spirally inwards. In the last edition of the Encyclopædia Britannica, Sir John Herschel, in the article Meteorology, (73), gives his assent to the views of Professor Taylor, by which the veerings of wind are explained on the supposition that the air must flow spirally towards a centre and *ascend.* These views of Taylor were laid before the British Association at Glasgow, in 1855, when their author explained that he considered Espy's views were untenable, inasmuch as a low barometer could not be maintained in the centre of storms, whilst the winds continued to blow towards the area of minimum pressure. But we submit that both Professor Taylor and Sir John Herschel must fall back upon Espy's mode of explanation; for, by their own reasoning, the winds would blow *directly* to a central space, if the earth had not moved on its axis.

To show the dilemma in which the advocates of the rotatory theories are placed, when endeavouring to explain the observed phenomena of storms, let us first have clear ideas of the points on which Reid and Redfield agree.

Rotatory storms are supposed to be generally of great extent in extra-tropical latitudes, according to Mr Redfield, sometimes 2000 miles in diameter. Now, it is evident, that whether a rotating and progressive storm have a diameter of one mile or two thousand miles, there must be a stream or current of air of as great breadth as the diameter of the whirlwind storm, to bear it along and to regulate its rate of progression. It is not possible that rotation and translation can take place in any other circumstances.

A large body of air in a state of rotation and translation must be borne on in the general current in the same way as a balloon is in the stream of air in which it floats. Revolving gales of wind in a state of translation could only exist with such a broad stream of air acting as its precursor and vehicle.



Thus, if a storm AB, having a diameter of 500 miles, is progressing at the rate of 25 miles an hour, then there must also be a current CD, having a breadth of 500 miles at least, and blowing at the rate of 25 miles an hour. In regard to this point, Mr Redfield says, with truth, "*That the progression of rotatory storms is caused by the predominant current in which they are imbedded, appears nearly a self-evident proposition,*" simply because such a mass of air could not force its way through, or displace the air in front of it. We could as soon suppose that a storm in a state of *rotation and translation* might proceed against a current of wind moving at the rate of 10 miles, as that it could make its way through air in a state of rest at the rate of 20 miles an hour.

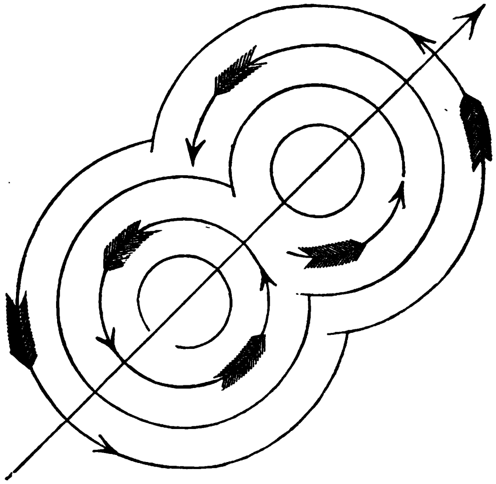
But it is a characteristic of storms in the tropics, and it is usually a characteristic of storms in the temperate latitudes, that a calm precedes them. The air in front of what have been supposed to be rotatory gales is commonly in a state of rest. The greatest of all observers says—

> "We often see, against some storm,
> A silence in the heavens, the rack stand still,
> The bold winds speechless, and the orb below,
> As hush as death."—HAMLET.

Instead of a current of wind preceding the storms that are supposed to be vast whirlwinds, there is a mass of air to remove. What, we would ask the advocates of Reid or Redfield's rotatory theory, becomes of the air at rest in front of their revolving storms?

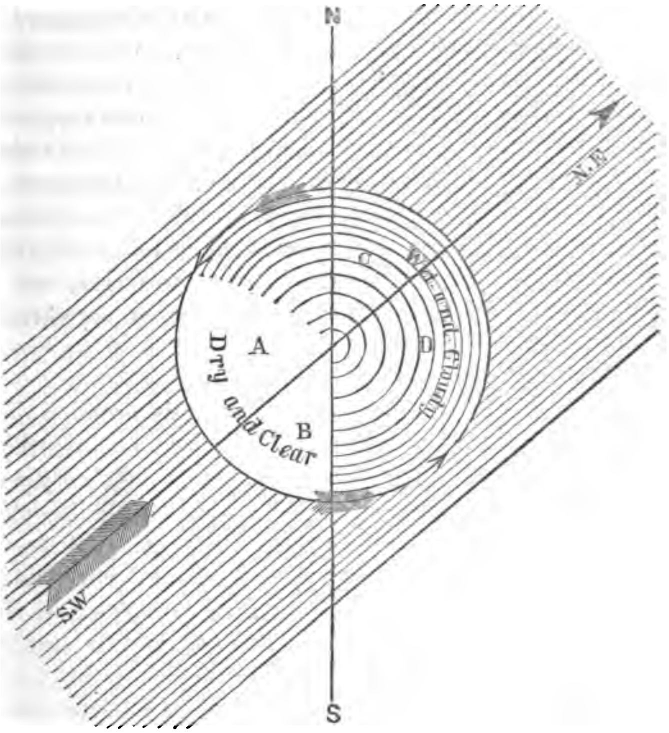
So much, apparently, did the difficulty of giving an explanation of the manner in which the air in front of rotatory gales is disposed of present itself to Sir John Herschel, that he at one time proposed another theory, which differs entirely from the rotation-and-translation one of Reid and Redfield. He considers that, although the air of storms revolves round a centre, and by its centrifugal force causes the fall of the barometer, the revolving mass is not *translated* from one place to another. Writing of tropical hurricanes, he says,—"They consist of a revolving movement, propagated from place to place, not by bodily transfer of the whole mass of air, which at any moment constitutes the hurricane, from one geographical point to another, but by every part of atmosphere in its track receiving from that before it, and transmitting to that after it, this revolving movement." It is somewhat difficult to conceive how such a congeries of independent circles of air could be maintained, and, if we mistake not, Sir John Herschel has already abandoned this hypothesis.

Far less, however, can we give our assent to the supposition that two rotation-and-translation storms could come in contact by the one overtaking the other. Physical objections of the same nature as we have already stated in regard to these particular storms moving onwards and revolving through a mass of air at rest, apply still more strongly to the proposition as stated and illustrated by Sir William Reid. It would scarcely be possible to adduce stronger arguments against the rotatory theory, and its utter inadequacy to cope with the explanation of phenomena, than his statement, that "gales succeed each other so fast, when passing over the British Islands in the winter season, that it is not easy to identify any particular gale which it may be desirable to study. As storms proceed northward, even if they do not increase in diameter, they may be expected to meet, owing to the contraction of the meridians, and to neutralize each other on the sides in contact, as represented in this figure."



"From the same cause, gales may subdue each other, and subside. If we conceive two gales of great extent to co-exist on the same latitude, one of them on the meridian of Greenwich, and the other on the thirtieth degree of west longitude, which is the middle of the Atlantic, and conceive both to be moving north at the same rate of progression, they would meet; when gales follow in close succession, overtaking each other, the one may have the effect of neutralizing the other as in the figure."

There is another objection to the rotation-and-translation rotatory theory, which applies with great force, more especially to extra-tropical storms. Why do there exist so great differences in the temperature of the air supposed to be in a state of rotation and translation ?



Let ABCD be a rotation-and-translation storm, imbedded in a current flowing from S.W. to N.E., it must appear very curious why the front part CD is often wet, warm, and cloudy, while the rear AB is dry, clear, and cold. The most northerly part of the supposed whirlwind is not the *coldest;* it is the *south-westerly.* These differences in the temperature of the southerly and westerly winds are often great in Britain, but not nearly so great as in North America, where it is not uncommon for the thermometer to sink 60 degrees of temperature in the course of 24 hours, on the changing of the wind in winter.

The rotatory theory of Professor Dove now requires to be noticed, as it is quite peculiar. The centripetal theory of Espy agrees much more closely with the rotatory theory of

Taylor, than the rotatory theory of Redfield does with the rotatory theory of Dove.

It is thus seen that the rotatory theories of Reid, Redfield, and Herschel, are totally distinct from each other; and to each grave objections can be urged, when it is attempted to deal with the actual phenomena of storms. In a previous number of this Journal (October 1856), we showed that the remarkable storm of 6th and 7th February 1856 was altogether inconsistent with the idea of rotation, for, instead of the wind blowing at right angles to the area of minimum pressure (as would have been the case had it been revolving round a centre of minimum pressure), it blew towards the area of minimum pressure. As an instance of the obstruction that arises to the cause of science from the prevalent practice of referring back to authority, we give the remarks of a critic in the *Scotsman*, in noticing our paper on this storm.

"The author asserts that the storm on this occasion had not a rotatory character; and some of the facts mentioned go far to prove that, taken in this peculiar form, the theory will not apply. When, however, we take the more philosophic views of Dove of Berlin, that the winds in the temperate zone are the contests of the opposing north and south currents in the atmosphere, and that the *rotation* is merely one of the results of this contest, the facts mentioned by Mr Russell seem by no means difficult of explanation."

We only wish that our critic had made the attempt to explain the phenomena by Dove's theory, which we will shortly show is as inconsistent with facts as any other of the rotatory theories that have been proposed. We would remark, that it is altogether "unphilosophical" to talk about "opposing winds," simply because there is no such thing as one wind opposing another. But in the very next sentence, our critic almost confesses that he has great doubts if Dove's theory will do; for he is yet in search of "the law" of storms.

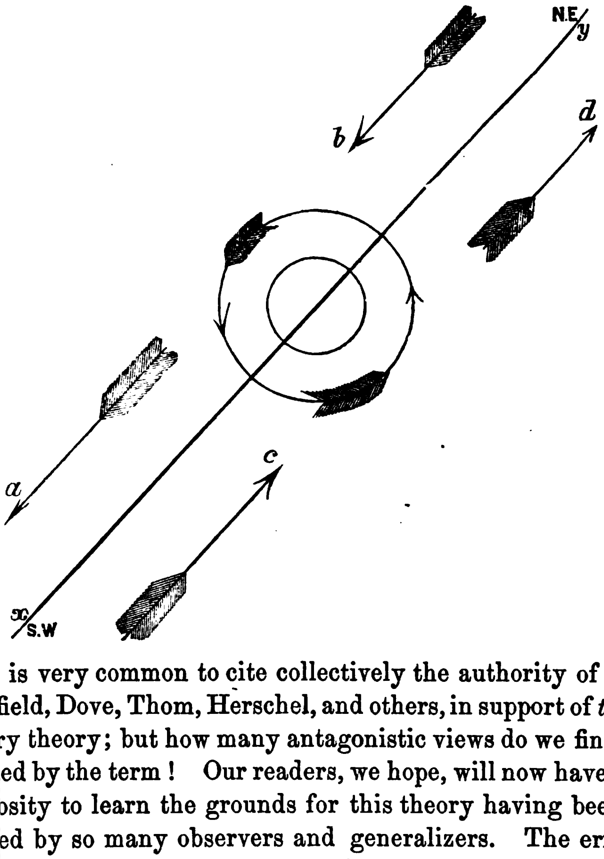
"One of the important results that we may expect from the Meteorological Association for Scotland will be fuller means of verifying these and other theories of winds, which, however, we may here remark, will require to be tested, not by storms, but by the more ordinary changes of the atmosphere. Here, as elsewhere, we believe that *the law* will be found in ordinary every-day events sooner than in the extraordinary and anomalous."

But storms do afford a much better means of testing or verifying the true "law," simply because, when the disturbances are greatest, they are susceptible of being more readily measured by our instruments. The imperfections of our instruments, indeed, do not enable us to trace the principles upon which the more ordinary motions of the air depend. Even in the case of the land and sea breeze, our barometers are not delicate enough to indicate the differences of the pressure over the land and over the sea to which the breeze owes its force. It is universally admitted, however, that the law, in the case of the sea-breeze, is, that the air is propelled from a higher towards a lower barometric pressure. Surely there is nothing "unphilosophical" in the assertion that the same law may hold good in the case of storms; in fact, "in the extraordinary and anomalous," as well as "in ordinary every-day events."

The rotatory theory of Dove may be called the parallel-current theory. Instead of the whirlwind storm being imbedded in a broad aerial current, as Reid and Redfield maintain, Dove alleges that the whirlwind is produced by two currents, which are supposed to flow side by side.

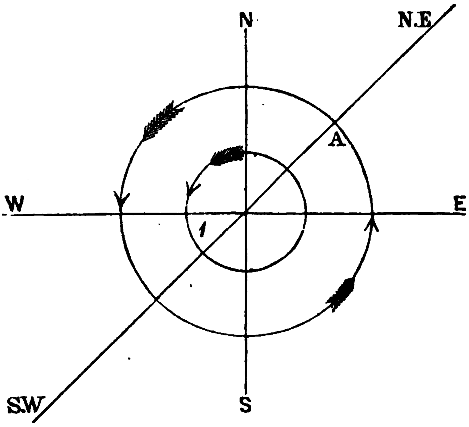
He has been led to believe that there are only two atmospheric currents in all latitudes—the one polar, the other equatorial. In the tropics the one stratifies over the other; in the temperate latitudes they flow side by side. By this view the trade-wind of the northern tropic is overlaid by a *south-west* current. On the other hand, a *south-west* wind in our latitude should indicate that a *north-east* wind should be blowing in another longitude, to restore the balance of air flowing from the southern quarter. He supposes that rotatory storms are induced by the contact of two currents flowing side by side.

Thus, if we suppose *a b* is the north-east or polar current, and *c d* the *south-west* or equatorial current, flowing side by side at *x y*, a whirlwind storm revolving from right to left will be the result of these parallel currents moving in opposite directions. One might endeavour to explain the extraordinary differences in the temperature of the different winds in storms on Dove's hypothesis, did there not exist great objections to the idea that storms originate from parallel currents. The well-known fact that our winter gales are usually preceded by a calm, completely disproves the existence of such currents, which, we are glad to say, are not recognized by Reid and Redfield.



It is very common to cite collectively the authority of Reid, Redfield, Dove, Thom, Herschel, and others, in support of *the* rotatory theory; but how many antagonistic views do we find embraced by the term ! Our readers, we hope, will now have some curiosity to learn the grounds for this theory having been accepted by so many observers and generalizers. The error of observation which has been committed will one day be looked back upon as one of the curiosities in the history of meteorology.

It is admitted by all parties, that in this latitude the wind, during storms, often veers from *south-east* to *north-west.* All the advocates of the rotatory theories assert that this can only be accounted for on the supposition that a revolving storm, having a progression from *south-west* to *north-east,* has passed over the spot where the wind has veered from *south-east* to *north-west.* Thus, if a whirlwind passed over a spot at A,



from south-west to north-east, the wind would set in from the south-east, and suddenly change to the north-west, when the centre passed over A. This appears so simple an explanation of a common phenomenon, that the rotatory theory has been founded upon it. To explain such a veering in the wind, it is *necessary* to assume that the storm has a progression from *south-west* to *north-east.* For example, if a whirlwind storm had a progression from *west* to *east,* and if the wind set in at *south-east,* it would veer to *north-east* as the storm passed over any spot, and also from due south to due north. Such veerings rarely happen, however, and hence rotatory theorists have assumed that all storms must have a progression from *south-west* to *north-east.* The theory is grounded on this supposition, which is altogether a fallacy; inasmuch as it is now incontestably proved that the winter storms of North America and Europe have a progression from a point a little to the *north of west* to the *south of east.* This discovery entirely sweeps away the premises upon which the different varieties of the rotatory theory are founded.

Sir J. Herschel informs us, in his article in the *Encyclopædia Britannica,* that, " In the West Indies, they [rotatory storms] are confined to a pretty definite area; their usual course being in a parabolic curve, having some point near Bermuda for its focus—originating in the Gulf of Florida—and running along the coasts of the United States, following generally the course of the Gulf Stream." In order to promote the advance of meteorology, we would anxiously ask this eminent man of science to re-examine this question; for there is an overwhelming mass of evidence to show that storms do not generally follow the course of the Gulf Stream. Elsewhere we have already pointed out the errors in the mode of observation which have led Redfield and others to the conclusion that storms have such a course.* All the American storms have an apparent progression from *south-west* to *north-east*, that is, along the course of the Gulf Stream or the coast, *if* observations are merely confined to either. This circumstance is readily explained by the fact, that the area of disturbance and low pressure of the barometer extends from north to south—being often about 600 miles in breadth, and upwards of 2000 miles in length. These elongated areas of atmospheric disturbances travel, or rather, according to our mode of explaining the whole phenomena, are propagated from *west* to *east* simultaneously across the eastern portion of the North American continent. For this reason the disturbances reach the Atlantic coast sooner in Florida than in Maine, because the former State is farther west than the latter, and not because it is farther south. All the atmospheric disturbances have thus an apparent progression from *south-west* to *north-east*, if observations are only taken at stations along the coast; but it is now well ascertained, in the case of the winter storms at least, that the true direction is from a point to the north of west to one south of east. Even Lieut. Maury admits,† and so agrees with Espy, Loomis, Hare, and others, that the winter storms of America travel in this direction. This admission, however, is fatal to the supposition that the winter storms are rotatory in their character, seeing that they usually begin as north-east winds in the north-eastern States; and it must be admitted to be physically impossible that a rotatory storm can commence with a north-east wind when its course of progression is from the *north of west* to the *south of east*.

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* North America: its Agriculture and Climate.

† Physical Geography of the Sea.

Among numerous instances that could be adduced of the extraordinary mode in which difficulties are explained when they stand in the way of the rotatory theory or theories, is the supposed great dilatation of the West India storms, when they reach the Atlantic coast of the United States. The area of disturbance, which is asserted to be sometimes 150 miles in the Gulf of Mexico, suddenly expands to a storm upwards of 1000 miles in diameter in the Atlantic. This phenomenon is easily accounted for when we remember that the atmospheric disturbances are apparently rapidly propagated along the United States as the wide elongated area of diminished pressure travels from west to east. Had the North American coast run due *north* and *south*, the atmospheric disturbances would have been found to occur simultaneously along the coast, and the rotatory theory would never have been applied to the storms of these regions.

The error of applying the rotatory theory to the winter storms of Europe which begin in many parts as *north-east* winds, is proved by the fact that they have a progression from the *north of west* to the *south of east.* It must be remembered that no rotatory storm can begin to blow from the north-east, and at the same time have a progression from *west* to *east.* What Sir J. Herschel and Mr Birt term "Atmospheric waves," whose crests they admit extend from N.N.E. to S.S.W., and having the direction of their progress from W.N.W. to E.S.E., prove that the winter storms of Europe have a course nearly at *right angles* to that which Dove and other rotatory theorists have inferred, from their having examined merely the changes at stations situated along a line running from S.W. to N.E., in which case the progression is only apparent from that quarter.

If meteorologists would lay aside the rotatory theory and discuss the phenomena of what are termed "atmospheric waves," we should entertain some hopes of the advancement of the science. "The atmospheric waves," says Sir J. Herschel, "were considered as those which originate, not from the general movement of the whole body of the atmosphere, but from internal displacements; the result of winds diverted from their course, or of great local disturbances of temperature, due to a concurrence of circumstances which may be termed casual, forasmuch as we cannot trace their laws."* This, at least, is an admission that the centrifugal motion of the air cannot account for these barometric depressions. And how can this be assigned as a cause, seeing the area of low barometer, instead of being circular, is often from three to four times of greater length than breadth!!

While Reid, Redfield, and many others, contend for the universality of rotatory storms as a means of accounting for the fall of the barometer and the veering of the wind, some consider that there are two kinds of storms. We have always held, however, that if you apply the rotatory theory to one storm, you must apply it to every breeze that blows. A letter from the President and Council of the Royal Society, to the Board of Trade, dated 22d February 1855, contains the following suggestion :†—

"It is much to be desired, both for the purposes of navigation and for those of general science, that the captains of Her Majesty's ships and masters of merchant vessels should be correctly and thoroughly instructed in the methods of distinguishing *in all cases* between the rotatory storms or gales, which are properly called *cyclones*, and gales of a more ordinary character, but which are frequently accompanied by a veering of the wind, which, under certain circumstances, might easily be confounded with the phenomena of *cyclones*, though due to a very different cause."

None ought to have been more fit to have given instruction as to the manner in which one kind of storm could be distinguished from another than the Council of the Royal Society; but they leave the subject as they found it, or rather make matters worse, by subscribing to the doctrine of *Cyclones*, and at the same time cautioning seamen against confounding them with other veering gales, without giving the slightest hint how such a thing is to be done. Have the Council of the Royal Society made up their minds whether their theory of Cyclones is consistent with the views of Reid, of Redfield, of Dove, of Thom, or of Herschel?

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* Encyclopædia Britannica, article Meteorology, page 651.

† Report of the Meteorological Department of the Board of Trade.

At the request of Professor Espy, Washington, I made an effort, along with Sir David Brewster, to obtain a committee of the British Association to inquire into the theory of storms, but did not succeed. The very contradictory opinions which are entertained in regard to the nature of the so-called rotatory storms, is surely sufficient to show the necessity of re-examining this question. I cannot conclude this paper, without giving a short extract from a letter received from Professor Espy, on learning that the British Association declined to entertain his proposal:—

"I am disappointed and grieved that the British Association did not appoint a committee to examine this subject. I wish you would prevail on Sir John Herschel to examine the decision which he made against my theory some seventeen or eighteen years ago, on the ground that the barometer did not rise in the centre of storms, as he thought it ought to do, if the wind blew inwards towards the centre; as my theory indicated. I know the result of such an examination; for he will immediately see that my theory explains, not only why the barometer is low in the middle of a storm, but why it continues to stand low, notwithstanding the wind blows inwards towards the centre. Sir John Herschel has sufficient reputation in various departments of science to enable him, without fear of injury to his high standing, to retract what he said on this subject many years ago, when the Association met at Cambridge. After examining the subject for himself, he might then think it an act of justice to use his influence to induce somebody of authority (the Board of Admiralty for example) to appoint a committee to examine the subject thoroughly, with the works of Redfield, Reid, Espy, and Piddington, before them; and determine, for the good of science and the safety of the mariner, what the facts in this case really are."

But as already stated, while Sir John Herschel and Professor Taylor, Glasgow, object to the principle of Espy's theory of storms, they have to fall back upon it if they attempt to explain why the barometer remains low in the centre to which the winds are blowing *spirally*. Yet although the belief in the rotatory theory has become prevalent in this country, through a great array of contradictory authorities, the truth is beginning to ooze out. Some years ago Mr Thomas Hopkins was a great opponent of Espy's views, and delivered a lecture to show their inapplicability to the storm of January 1839. This gentleman then hinted* that the phenomena might be explained by supposing that that violent gale was a descent of air from the higher strata of the atmosphere; now, however, in a paper which he read before the Royal Society on 19th March 1857, we find him an out-and-out supporter of Espy's theory, without any acknowledgment.

"In this paper it was maintained that the great disturber of the equilibrium of atmospheric pressure is the aqueous vapour which is diffused through the gases. The gases, when ascending, cool (say 5° through expansion, by diminution of incumbent pressure, whilst the vapour that is within them cools only 1°), and a consequence is, that when a mixed mass ascends, the vapour is condensed by the cold of the gases. It is well known that condensation of vapour gives out heat, and this heat warms and expands the gases, when they are forced to ascend, taking vapour with them; and the process being repeated and continued, an ascending current is produced in the atmosphere, cloud is formed, the barometer sinks, rain falls, and wind blows towards the part."†

The advocacy of Espy's theory of storms by an old opponent is certainly very encouraging. The principle, as stated by Hopkins, is theoretically impregnable. We have no doubt that it is the mode in which the violent winds of the tropical seas are produced, for a careful examination of observations shows that the winds are in-blowing, In regard to the winds of extra-tropical latitudes, we maintain that a modification of Espy's theory best accords with all the phenomena. In another paper we shall explain upon what points we agree with Espy, and upon what we differ.

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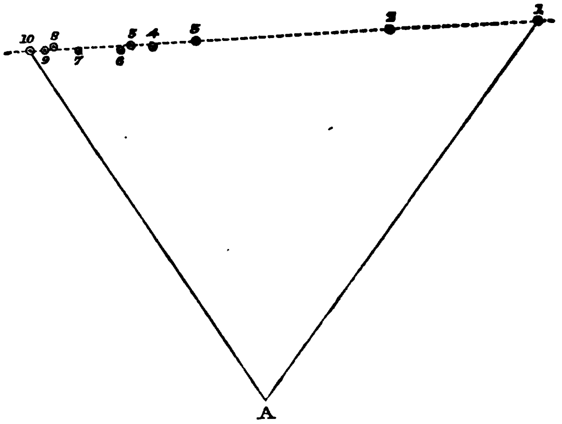
* Espy's Philosophy of Storms, page 485.

† Edinburgh and London Philosophical Magazine, for October 1857.

Relative Path of the Components of 61 *Cygni.* By Captain W. S. Jacob, H.E.I.C., Astronomer.



Scale, 4″ to one inch.

1. Bradley,	1753.	6. Smyth,	1837.
2. W. Herschel,	1780.	7. Jacob,	1847.
3. Struve, H. & S.,	1821.	8. Do.	1851.
4. Dawes,	1830.	9. Do.	1853.
5. Do.	1834.	10. Do.	1857.

The above diagram represents the relative motion, for a period of 104 years, of the two stars composing the pair called 61 Cygni. This pair is remarkable for its very large proper motion, exceeding 5″ per annum, and has also been long considered as one of the binary systems, the periodic time being supposed to exceed 500 years. An inspection of the diagram will show that this view must be erroneous, since the relative path of the two stars is a straight line. (A is the place of the principal star considered as a centre, and $\overset{1}{\odot}\overset{2}{\odot}\overset{3}{\odot}$ &c., the successive places of the other star). The mutual action of the two stars is therefore insensible, and they will continue to recede from each other for an indefinite period. The mean annual parallax of the pair was determined by Bessel to be 0″·31, but as it would appear that the two stars are *not* very nearly at the same distance from us, it would be worth the while of any astronomer, having the means, to measure the parallax of each star independently, when the difference might perhaps be found a sensible quantity. W. S. J.

Madras Observatory, *October* 12, 1857.

Observations on British Zoophytes. By THOMAS STRETHILL WRIGHT, M.D., Fellow of the Royal College of Physicians, Edinburgh.*

DESCRIPTION OF PLATES.

Plate I.

Fig. 1. *Laomedea acuminata*, highly magnified—*a* polyp with tentacles expanded—*b* bud with growing polyp—*c* empty cell—*d* polyp disturbed—*e* capsule containing medusoid.

2. *L. acuminata*, magnified two diameters, to show the branched and unbranched states of the polypary.

Plate II.—*Laomedea acuminata.*

Fig. 1. *a* bases of three tentacles of polyp united by their connecting membrane, and studded with large thread-cells and masses of granules—*b* unconnected portion of tentacles, furnished with small thread-cells.

2. Ideal section of capsule containing medusoid taken at an early stage—*corallum, †ectoderm, ‡endoderm—*a* reproductive polyp—*b* medusoid inclosed within *c*, a sac formed by a layer of ectoderm. Circulation indicated by arrows.

3 and 4. Medusoid of *L. acuminata*, compared with fig. 5, medusoid of *Campanularia Johnstoni*—*a* tentacles—*b* rudimentary tentacles—*c* auditory capsules.

Plate III.

Trichydra pudica.

Fig. 1. Polyps—*g e c* in various stages of contraction—*f* with buccal cavity everted—*d b* extended—*a* young polyp.

Tubularia indivisa.

2. Transverse section of polypary near the summit—*a* corallum—*b* ectoderm—*c* endoderm pierced by *e* longitudinal canals.

3. Summit of polypary from which the polyp has recently fallen—*a* longitudinal spiral canals—*b* irregular transverse striæ, indicating the fall of successive polyps. Course of circulation marked by arrows.

Laomedea acuminata.

A beautiful zoophyte was discovered by Mr Alder, and described by him in the last December number of the "Annals of Nat. Hist.," under the title of *Laomedea acuminata*, which I am disposed to consider identical with the subject of this notice. It has been familiar to me since March last, when I found an old pecten shell in one of the tanks of my friend Dr Paterson of Leith, covered with its flower-like polyps. In May it was dredged up on an old oyster shell from the Frith of Forth, and sketched by myself and Dr Mackay; and in August, a fine specimen occurred on a living oyster in the vivarium of the Edinburgh Zoological Gardens, which has been domesticated with me ever since, and which I place on the table to-night. Mr Alder describes it thus: "*Laomedea acuminata*—Polypary minute, scarcely branched, with a slender annulated stem; cells thin, membranous, finely striated longitudinally, elongo-ovate or pod-shaped, squared below and tapering to a fine point above; margin slightly crenulated; polyp reaching, when extended, to two or three times the length of the cell, with about twenty muricated tentacles." He remarked, also, that the tentacles were united by a web for about one-sixth of their length, which he has well shown in his figure of the polyp. In all the specimens in my possession the tentacles, instead of being erected as in Mr Alder's figure, were alternately erected and depressed (fig. 1, *a*), as they reached the top of the membranous funnel which united their bases together.

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* Communicated to the Royal Physical Society, on the 25th March 1857.

The distinguished discoverer of this Zoophyte found much difficulty in ascertaining the true shape of the margin of the cell, on account of its exceedingly thin and membranous texture. This membrane, however, appears to me to be an additional softer structure, which incloses the cell proper, and, projecting beyond the mouth, falls twisting together when the polyp retires within its cell. Old cells, accordingly, which have long lost their tenants, are destitute of this membrane, and present an even rim like old cells of *Campanularia syringa*. In my specimens, moreover, the cells were inclined to their annulated stems. The long lax tentacles were muricated with *small* thread-cells, while the inner surface of the membranous web or funnel was studded with thread-cells of very large size, ranged along each side of the tentacles. (Pl. II. fig. 1.) Similar large cells were also found scattered on the body of the polyp.

In September last buds were put forth from the foot of many of the polyp stems, which became slowly developed into cylindrical capsules, supported on long pedicles, and of large size compared with the minute polyps to which they were attached. As I was obliged to leave home at that time, I examined the capsules, and found in each a single large Acaleph or medusoid, imperfectly developed within a fleshy sac, which was thickly covered with large thread-cells. During the present month similar capsules again appeared; and Alcmena never wearied more of her prolonged gestation of Hercules than I did, as day after day these capsules slowly increased in size and revealed the young giant within. At length fierce throes commenced; but Latona sat cross-legged at the threshold for a night and a day before the sac burst, and a pale-emerald green medusoid was brought forth. *The umbrella* of the Acaleph is colourless, and sub-hemispherical, becoming mitrate during contraction. It is covered with the *large* thread-cells, which are congregated in greater numbers about the middle and upper parts, and give the animal a shiningly dotted, or gemmed appearance. *The sub-umbrella* is tinted with pale emerald-green by reflected light, and is colourless or faintly orange by transmitted light; effects probably due to interference of light produced by the fibrous structure of its highly-developed contractile layer. *The stomach or alimentary polyp* is quadrangular. *The tentacles* or *prehensile polyps* are four in number—two long and two rudimentary; they are ringed as to their bulbs with deep blue, and are without eye-specks. *The auditory capsules* are eight in number, situated one on each side of the four tentacles. The tentacles and alimentary polyps are furnished with small thread-cells. The Acaleph has no ovaries or sperm-sacs.

The general appearance of this Acaleph resembles that of the Acaleph of *Campanularia Johnstoni*, Alder, of which Mr Gosse has given a figure in his "Devonshire Coast."

Great numbers of the Acalephs of *L. acuminata* were given off at the same time, and after living a few days, became affected with the convulsive attacks so feelingly described by Professor Edward Forbes, to which infant Acalephs are so prone, and died in contortions shocking to see.

[Since the foregoing observations were communicated to the Royal Physical Society, I have several times obtained *L. acuminata*, and it is now growing in great luxuriance in my tanks. One of the specimens covers a space of 4 by 8 inches on the surface of the glass, with a net-work of creeping fibres, from which polyp-stems spring at very regular intervals of about a tenth of an inch. The polyp-stems of this specimen bear each a single polyp only. In other specimens which are seated on univalve shells, and cannot therefore so readily spread themselves, the polyp-stems become repeatedly branched. In these cases the single polyp-stem gives off one, two, or three branches beneath its cell; these branches in like manner originate others, until the polyp-stem becomes transformed into a more or less bushy shrub, covered with polyps (Pl. I. fig. 2), and rarely bearing a large medusa-bud, which is generally developed from the first stem.

The medusa-bearing stem (Plate II. fig. 2) at an early stage resembles one of the ordinary polyps (Plate I. fig. 1, *b*), in an imperfect state of development, having the same transparent globular summit, in which, as well as in the stem, an active circulation of granules may be detected. It may be considered as a reproductive branch or polyp. The medusoid *b* buds forth from beneath the enlarged head, and is inclosed in a sac *c* formed from the ectoderm of the polyp. As the medusoid grows, first the head, and afterwards the body, of the reproductive polyp *a* is absorbed, and the sac of the ectoderm is afterwards ruptured by the vigorous flapping of its inmate. Absorption of the connection between the stem and the medusoid then takes place, and the latter is freed in about six or eight hours afterwards.

The striæ of the empty polyp-cells appear to be due to a folded state of the membrane, as they disappear when the cells are fully distended by their inmates.—Dec. 3, 1857.]

Trichydra pudica.

Shells and stones which have been kept quiet in an aquarium for some time, are occasionally covered with a flocculent net-work of shining fibres, which appear as fine as the lines of a spider's web. This net-work, under microscopic power, is found to be composed of the interlacing tentacles of a multitude of closely-congregated polyps, attached together by a linear creeping polypary (Plate III. fig. 1). The polyp of this minute Zoophyte (which I have called *Trichydra pudica,* "the modest hair polyp") is about ⅛th of an inch in length, and resembles in shape a miniature fresh-water hydra. The whole body is exceedingly attenuated and transparent, with the exception of the buccal cavity, which is of a dense silvery white, and may be distinguished by reflected light as a shining speck, while the rest of the animal is almost invisible. The tentacles vary in number from 4 to 12, with the increasing age of the polyp. They are arranged in a single row, and are long and waving, and muricated with clusters of minute thread-cells, from which project long and finely acuminated "palpocils," the soft prehensile spines I have described in former communications. The buccal cavity is small and conical, and occupies a scarcely elevated papilla situated in the centre of the tentacular circle. Its walls are exceedingly dense, and open superiorly by five motile lips. The buccal cavity is frequently everted as a flat disk, when the tentacles are depressed along the body *f*. For a long time I considered that the polyps were naked and single, as I was unsuccessful in detecting either a connecting polypary or a corallum, while the Zoophyte remained *in situ*, and any attempt to remove it caused the polyps to disappear altogether. Afterwards, the stones on which they grew became coated with fine dust, deposited from the water, and afforded no hold for the creeping polypary; the latter, therefore, floated unattached as tortuous white threads bearing polyps. The polypary was inclosed in a transparent membranous sheath or corallum, which at intervals bore short, cylindrical, even-rimmed cells of unequal length, for the reception of the polyps. This interesting little zoophyte is remarkable for the laxity of its habit, and the extensibility and transparency of its polyps, arising from the extreme vacuolation of their tissues. When at rest the polyps extend their bodies and tentacles to their utmost length; but a sudden glare of light, or shaking of the vessel in which they are confined, causes the modest hair polyp to contract itself, or to bend the buccal cavity and tentacles loosely downwards, like a flower drooping on its stalk. It seldom entirely withdraws itself into its cell unless irritated.

I have never observed any reproductive apparatus or acaleph-bearing capsules on this zoophyte; and, in default of their appearance, I am disposed to class it with the Corynidæ of Johnston; and that on account of the progressive development of the tentacles, which, as in Coryne, Clava, and Hydractinia, become more numerous with the increasing age of the polyp, while in the Campanulariadæ, to which I at first referred it, under the name of *C. trichoides*, the growing polyp has its full complement of tentacles when it issues from its opening cell. The polyps of Trichydra also differ from those of the Campanulariadæ and Sertulariadæ generally, in showing no disposition to hold the tentacles in a double row; an arrangement of these organs which has not been sufficiently noticed in the figures and descriptions of authors on these classes.

*On Tubularia indivisa.**

The object of this notice is to elucidate some points in the anatomy and physiology of *Tubularia indivisa* which have escaped detection by, or presented difficulties to, the numerous authors who have written on this zoophyte.

This species of Tubularia, as many members of the Society are well aware, is common in the Frith of Forth, where it is dredged up from the oyster-beds in considerable quantities. It resembles, as Ellis has remarked, an oat-plant with the straws topped or truncated at from two to eighteen inches from the root, each stem bearing at its summit a single polyp of a white, pink, or rich crimson colour, and furnished with a double row of tentacles. In describing the anatomy of this zoophyte, I shall take the different parts in the order I have observed in my communication on the anatomy of Hydractinia, viz., 1*st*, the corallum; 2*d*, the polypary; and, 3*d*, the polyp. *The corallum*, or polypidom, is a simple yellowish chitinous tube, straight or slightly flexuous. It is often divided at the base, so as to form sinuous *quasi* roots, which creep over shells and stones, and occasionally the coralla of other zoophytes, resembling, as Ellis quaintly observes, "the guts of small animals." Many tubes are often found twisted together by the roots. The tube of the corallum increases in diameter from its attachment upwards, and is marked at irregular distances by wrinkles or annulations. The chitinous substance is brittle, cutting cleanly between the scissors, without splitting, but its illuminating action on the dark field of the polariscope indicates that it is composed of fibres running in a longitudinal direction.

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* Communicated to the Royal Physical Society, 27th April 1857.

The polypary, or that part of the animal which is inclosed within the corallum, presents a structure of great interest. Johnston describes it as a soft, almost fluid, reddish-pink pulp or medulla in organic connection with the polyp. Dalyell states that the tube is replete with a yellowish tenacious mucous matter completely occupying the whole, or accumulated in irregular ruddy masses. These naturalists were therefore ignorant of the anatomy of the polypary, though Johnston remarked that the recent stalk was marked by longitudinal pale lines placed at equal distances, which he justly considered were evidences of some peculiar structure in what he termed the interior pulp; and he inquires, "What is their relation to the currents observed by Mr Lister?" It is probable that Johnston referred from memory to Lister's discovery of the circulation in Tubularia, as the latter writer, in the 124th volume of the Philosophical Transactions, clearly describes these lines, and their relations to the currents. He remarks, "when magnified about one hundred times, a current of particles was seen within the tube that strikingly resembled, in its continued steady flow, the circulation in plants of the genus Chara. The general course of the stream was parallel to the slightly spiral lines on the tube. On the greater part of the side first viewed, it set as from the polypus; but on reversing the glass trough so as to show the other side, the flow was there towards the polypus: each current thus occupying half the circumference." "The tube had, between the lines of more conspicuous spots, a granular appearance, and beneath this the currents ran." Dalyell, though he examined a great number of specimens of all sizes and ages, was never able to detect any such circulation, and appears strongly to doubt, although he does not deny, its existence. It certainly is not readily observed in healthy individuals, as the moving fluid is very clear, and generally contains little or none of the granular matter which is carried along by the circulation in most of the hydroid zoophytes. Its existence, however, indicated by the passage of a few flying particles, may be detected in all living specimens, especially in those which have cast off their polyps, and in which the process of the renewal of those organs requires the conveyance of solid matter to them from all parts of the stem. Lister's observations were conducted on a single specimen which he had found thrown up on the sea-shore, and in which the polyp was in course of being absorbed, and its solid matter stored in the circulating fluid for the production of its successor.

On first obtaining a favourable specimen of *Tubularia indivisa,* I directed my attention to the structure of the polypary, and the phenomena of the circulation within it. I found that each of the spiral lines was generally formed of two narrower lines running close and parallel to each other (Plate III. fig. 3), and that the circulation took place along the wider interval between the double lines. These intervals had the appearance of canals situated immediately beneath the corallum, and occasionally communicating with each other by cross branches. A thin transverse section of the stalk, readily made by the aid of a fine pair of scissors (Plate III. fig. 2), showed that (with the exception of a thin layer of "ectoderm" *b*, which lined the inside of the corallum *a*), the whole of the tube was filled with a highly-vacuolated or cellular "endoderm" *c*, having the appearance of the pith in the section of an exogenous plant, and was generally impervious to the passage of fluid. Immediately within the ectoderm, the endoderm was perforated by eight or more equidistant canals *d*, finely ciliated in their interior, and having their walls loaded with coloured granular matter. The interstices between these canals corresponded to the double lines seen in the longitudinal view (fig. 3). As the polypary emerged from the corallum, the tubes became wider, and opened into each other until they formed a single cavity immediately beneath the lower range of tentacles of the polyp; here the circulation became influenced by a mechanical provision, hereafter to be described. The circulation in *Tubularia indivisa,* therefore, as far as relates to the polypary, is carried on by ciliary motion in canals which permeate the periphery of the endoderm in the longitudinal direction of the stem. The movements in the different canals are not related; in some of the canals the fluid is passing upwards, in others downwards, and in others it is at rest, previous to its commencing to flow in an opposite direction.

The polyp of Tubularia is distinguished by two rows of filiform tentacles,—the one short and fringing the mouth, the other long and forming a circle round the base of the buccal papilla. The buccal papilla is striated by crimson longitudinal markings, produced by aggregations of the coloured granular matter of the endoderm, and generally continuous with lines passing upwards from the spiral tubes of the polypary. In healthy specimens, the buccal papilla is constantly slowly dilating, or contracting, and pumping the fluid contained in the polyp backwards and forwards alternately between its own cavity and that which exists below the tentacles, and which, as I have stated, is formed by the anastomosis of the spiral tubes of the polypary. Hence Dalyell has called the polyp the heart of the zoophyte. And one might, though incorrectly, call this the cardiac circulation, and that of the polypary the capillary circulation of the animal.

In specimens kept in captivity the flower-like polyp generally drops off, and is renewed every four or five days, and at each renewal a ring, sometimes a circular spathe, is formed by the tip of the old corallum, as the corallum secreted by the growing polyp rises up within it. Hence, the length of the interval between the rings indicates the age which has been attained by each successive polyp.

I have already stated, that three modes of reproduction occur in Hydroid zoophytes. 1*st*, Oviparous; 2*dly*, Larviparous ; and 3*dly*, Polypiparous, in which last the young become developed into complete polyps before leaving the ovarian sac, as in the zoophyte we are now considering.

The female reproductive process in Tubularia has been investigated by Baster, Dalyell, and Van Beneden, and their researches have been confirmed by Mummery. The ovarian sacs are attached to stalks which spring from the base of the buccal papilla, above and close to the lower tentacles, and between the crimson striæ, and resemble bunches of grapes hanging down on all sides. They are frequently developed in such numbers, and attain so great a size, as to almost hide the polyp in their clusters. The stem of the cluster, and each of the grape-like ovisacs, is formed of the usual three (ectodermic, muscular, and endodermic) layers, and in each ovisac a single ovum, or sometimes two ova appear, which become developed into perfect polyps, are extruded from the summit of the ovisac, and fixing themselves by their base, commence the development of a polypary, like the parent zoophyte.

In male specimens, for this zoophtye is dioecious, the spermatic capsules resemble in shape and structure the ovarian sacs of the female, except that instead of ova, we have a gelatinous plasma, secreted between the endoderm and muscular layer, in which spermatic cells, and afterwards spermatozoa, are developed. The spermatozoa of Tubularia were discovered by Krohn in 1845. Their existence, of which there is no room for doubt, has since been denied by Van Beneden and Johnston.

REVIEWS AND NOTICES OF BOOKS.

The Rambles of a Naturalist on the Coasts of France, Spain, and Sicily. By A. DE QUATREFAGES, Member of the Institute, Professor of Ethnology in the Museum of Natural History, Jardin des Plantes, Paris, &c. Translated, with the Author's sanction and co-operation, by E. C. OTTÉ, Honorary Member of the Literary and Philosophical Society of St Andrews. 2 vols. post 8vo. London, 1857.

In these volumes we have an admirable translation by Miss Otté, the accomplished translator of Humboldt's Cosmos, of a French work in many respects of great interest, by the well known naturalist whose name it bears. The articles composing it formerly appeared in the *Revue des deux Mondes*, but the author's desire to commend the pursuit of sciences so dear to himself to unscientific readers, has led him to publish his papers in their present form, with copious notes, and an appendix, enriched by sketches of the lives of authors to whom he refers.

M. de Quatrefages is one of those enthusiastic lovers of nature who cannot fail to attract very many in their train. Nature is truly to him a mother, to whom he may flee in trouble, sure of a kindly welcome; and who, after soothing her child, will unfold to him certain of her wonders, knowing that in their search he will leave the sting of grief behind. Let us hear his opinion of this. "If you still preserve any of those illusions which, day by day, are vanishing amid the turmoils of life, if you regret the dreams that have fled never to return, go to the ocean side, and there on its sonorous banks you will assuredly recall some of the golden fancies that shed their radiance over the hours of your youth. If your heart have been struck by any of those poignant griefs which darken a whole life, go to the borders of the sea, seek out some lonely beach, an Archipelago of Chaussey, or an Isle de Brehat, beyond reach of the exacting conventionalities of society; and when your spirit is well-nigh broken with anguish, seek some elevated rock, where your eye may at once scan the heaving ocean and the firmament above; listen to the grand harmonious voices of the winds and waves, as at one moment they seem to murmur gentle melodies, and at another to swell in the thundering crush of their majesty; mark the capricious undulations of the waves, as far as the bounds of the horizon, where they merge into the fantastic figures of the clouds, and seem to rise before your eyes into the liquid sky above. Give yourself up to the sense of infinitude, which is stealing over your mind, and soon the tears you shed will have lost their bitterness; you will feel ere long that there is nothing in this world which can so thoroughly alleviate the sorrows of the heart as the contemplation of nature, and of the sublime spectacle of the creation, which leads us back to God." (P. 120, vol. i.)

Miss Otté is a kindred spirit, able to sympathise with our author's joy in finding a new or rare specimen; able also to appreciate his scientific researches and original discoveries: and in the society of these two gifted minds, we gladly join in spirit the excursions and enterprises they communicate to us.

M. de Quatrefages appears in these pages, not as wedded to one science, as is sometimes the case with men of his class, but rather as having a fraternal affection for them all. Though marine zoology was the special object of his devotion, yet natural history in all its branches, geology, and botany, has each a high place in his records; while his eyes are open to all that is novel, curious, or instructive around. Lighthouses, mussel-beds, volcanoes, historical associations, with pleasant or unpleasant travelling incidents, are by turn brought before us, and of each he has something to say full of interest. Avoiding technical details, he yet never allows himself "in the slightest degree to sacrifice the substance to the form;" so that, while enjoying his rambles, we can place perfect confidence in the strictly scientific accuracy of his descriptions of natural history.

The longest excursion noticed was to the coasts of Sicily, and M. de Quatrefages, with two gentlemen, the celebrated M. Milne-Edwards, and M. Blanchard, formed a scientific commission appointed by Government. Previously he had spent a considerable time in the Archipelagoes of Chaussey and Brehat, lying off the coast of France, at the entrance of the Channel; and lastly, he gives us the results of visits to the coast of the Bay of Biscay, and that of Saintonge.

He justly supposes that the great problem of life, so puzzling to physiologists, may be solved in part by help of the inferior creatures, whose transparent bodies often admit the actions of the various organs to be visible in a way impossible in the higher organisms.

The submarine excursions of one of M. de Quatrefages' party in the visit to Sicily, are worthy of special notice. By means of an apparatus having a flexible tube passing from a metallic helmet to an air-pump, M. Milne-Edwards was able to remain for nearly an hour at a time under water, collecting and observing the zoophytes which live from 10 to 13 feet below the surface of the water. These hazardous experiments were rewarded by important discoveries respecting the embryology of Molluscs and Annelids.

At La Rochelle, M. de Quatrefages had the opportunity of observing the organization of that remarkable parasite of the Torpedo, the Branchellion, a worm from an inch to an inch and a-half in length, which is undisturbed by shocks so powerful as sometimes to compel the fishermen to drop the net in which a Torpedo happens to be. For particulars of this, and many other important investigations, we must refer our readers to the volumes before us. They will thank us for introducing to their notice such a storehouse of facts. The appendix enters more fully into minute details, and gives abundant references. We feel grateful to Miss Otté for the labours which yield us such fruit.

M. de Quatrefages has recently been appointed to the chair of Ethnology at the Museum of Natural History in the Jardin des Plantes, and he concludes by a gentle reference to the pain it has cost him "to retire from the direct pursuit of paths of inquiry, which have yielded so many moments of unalloyed enjoyments."

Ueber das Verhältniss der Boghead Parrot Cannel-coal zur Steinkohle. Von H. R. GÖPPERT, Königl. Preuss. Geh. Medizinal-Rath und Professor zu Breslau. Berlin, 1857.

In the "Zeitschrift für das Berg-Hütten und Salinenwesen en dem Preussischen Staat, Vol. I.," is a Report by Professor Göppert of Breslau, "On the Relation of the Boghead Parrot Cannel-coal to Coal."

In this report Professor Göppert first describes, accurately, the generally prevailing views as to the origin of the coal measures; which he represents as derived from a luxuriant land vegetation of Coniferæ, Lycopodiaceæ, Ferns, Calamites, Sigillaria, Stigmaria, &c., submerged, and in process of time carbonized. The beds of coal are separated by deposits of sand or clay, which ultimately form sandstones and slaty rocks. These beds of coal, which are often very numerous in the same locality, have generally been deposited in still waters.

He considers bituminous shales to have originated *from the very same vegetable deposits*, only formed in waters somewhat agitated, and thus mixed with a large proportion of mineral matter.

He is of opinion that the presence of this foreign matter prevented the entire carbonization of the organic remains, such entire carbonization being only possible in beds of unmixed organic matter. Hence the combustible matter in shales is brown or yellow, with a brown or grayish-brown streak, and contains more hydrogen than that of true coal, the streak of which is black.

As the Boghead coal has a brown streak, and contains 25 to 30 per cent. of ash, he declares it to be a shale, not a coal.

We cannot accept this conclusion, which seems to be not justly deduced from the facts.

In the *first* place, although those who in Edinburgh supported the view that the Boghead mineral was not a coal, denied that it had the same origin as true coal, or that it contained remains of vegetable structure, Professor Göppert not only ascribes to it the same origin as he does to coal, but declares that the brown matter contains cells of the true coal-plants. As far, then, as origin is concerned, there is no ground for a specific distinction.

Secondly, as concerns the colour, it is known that all coals contain more or less of a brown matter, mixed with a black one, the latter, from its nature, determining the colour of the streak in most cases. But Professor Göppert himself speaks of gray, brownish-gray, and brown streaks, which are just what might be expected when the proportion of the friable black matter is diminished, or that of the brownish-yellow matter increased, beyond a certain point.

We may add, that the remains of cells are chiefly found in the black friable part of ordinary coals, and in the brown matter of shining cannel-coal; they are rare in the brilliant coal with conchoidal fracture, and in the dull varieties of cannel-coal, both of which, under the microscope, exhibit a dark brown, nearly opaque mass, with occasional yellow portions containing vegetable remains.

These observations indicate that all these varieties of coal are mere mixtures.

Thirdly, as to the amount of ashes. The published analyses of coal prove that the percentage of ash in true coals varies to a great extent, and is not confined to the limits of from 5 to 10 per cent., especially if we include cannel-coals, some of which contain as much, or even more ash than the Boghead coal. It is obvious that the amount of mineral matter beyond that belonging to the plants must have varied from the state of the water in which the plants were submerged, and of the rocks and soils contributing to form the mineral beds. Accordingly, we find the percentage of ash varying from 2 or 3 to upwards of 30 per cent. in undoubted coals. The Boghead coal, with its 25 to 30 per cent. of ash, is not a solitary case.

But Professor Göppert maintains that to admit the claim of Boghead coal to the title of a true coal will abolish all distinction between coal and shale. We do not think so; but what then? If, by his own account, the organic matter of coal and shale be identical in origin, but in shale it is less carbonized by reason of the presence of foreign impurity, is this a true, philosophical, specific difference? Even the true coals of Professor Göppert consist of brown matter and black matter; of yellow matter in uncertain proportions; and the amount of ash also certainly varies.

The question arises, Can we fix a point in the percentage of ash, where the mixture ceases to be coal, and becomes shale? We do not see that this can be done in the case of a series of minerals, all mixtures, except on one principle, namely, the quality of the mineral as a combustible. Whatever the percentage of ash, if the mineral has any value as a fuel, we should call it coal; if not, shale; considering neither coal nor shale as true mineral species, but as members of a series of mixtures, in variable proportions of vegetable matter, more or less carbonized, and in two, or rather three, different states—black, brown, and yellow, with mineral matter.

Professor Göppert illustrates the difference between Boghead coal (or shale) and true coal, by comparing them to what is called red, or imperfectly burned, and black, or perfectly burned charcoal. But surely we cannot consider an imperfect, half-made product, such as red charcoal (*charbon roux*), as a specifically distinct compound. It must, from the nature of the process, be a mixture; and no doubt many such mixtures might be obtained at different stages of the burning. But these cannot all be distinct; and how are we to select one as being so.

It is precisely so with coals and shales, if we attend to the presence of foreign mineral matter.

The series exhibits many stages, more or less complete, of carbonization in the organic matter (even the complete carbonization occurs, though in another formation, in anthracite); each stage yielding a mixture of several products of the imperfect change, these products occurring in most variable proportion, while the percentage of mineral matter is also highly variable.

Under such circumstances it is not easy to see how a true mineral species could occur. No such species does occur in this series, if by a true species be meant a definite compound. And if we felt disposed to constitute a mineral species, at what point is this to be done, and how is the species to be defined?

On the whole, it appears to us that the facts adduced by Professor Göppert do not support his conclusion, but rather confirm the views we have always held on this subject.

Memorials, Scientific and Literary, of Andrew Crosse, the Electrician. London: Longman & Co. 1857.

In this biography of Mr Crosse, the electrician, modestly and judiciously penned by his widow, we have a history of the life and death of a knowledge-seeking, simple-minded, and truly religious man.

Of Andrew Crosse it has been well said, by one who knew him intimately, "In him was indeed united the philosopher's head and the Christian's heart."

Possessed of a marvellous apparatus for extracting and retaining the electric fluid, the electrician brought down the lightning from the thunder cloud, or drew it from the November mist, and imprisoned that subtle yet mighty power in huge voltaic batteries and electrical jars, which poured forth their supplies into a large brass conductor, over which was inscribed the warning words, "Noli me tangere."

Broomfield, as described by Mrs Crosse, must have been a strange residence, surrounded with galvanic wires externally, and filled with galvanic batteries internally. It is beautifully situated among the picturesque Quantock Hills. There is a cave near Broomfield called "Holwell Cavern," where the traveller may rest awhile, and learn a lesson of the value of observation, and the cultivation of an observing mind. The walls of Holwell Cavern are effloresced with crystals of arragonite. Other men would have admired and passed on; the electrician determined to inquire by what process the crystals were formed. Many preserved the crystals as cabinet specimens; Crosse subjected the water which held their constituents in solution to the action of his voltaic battery. After an interval of ten days, he found that the negative wire was coated with crystals of carbonate of lime; and at the end of three weeks the whole of the salt was extracted from the cave water, and deposited at the negative pole. Acting upon this discovery, he prosecuted his researches, until, from his voltaic forge came forth "specimens of quartz, arragonite, chalcedony, carbonates of strontia, barytes, lead, and copper; sulphurets of lead, iron, copper, silver, and antimony, with many other compounds."

The history of the development of the "Acari Crossii" is well known, as also the persecution he underwent. So far as he would venture upon an opinion, Mr Crosse's solution of the phenomenon was, that they rose from ova deposited by insects floating in the atmosphere, hatched by the electric action.

It is, however, more especially to the FORMATION OF CRYSTALS under the power of the voltaic battery that we wish to direct attention. If these researches of Mr Crosse be followed with diligence and enthusiasm, what may yet be revealed to us respecting the SEGREGATION OF MINERAL VEINS, or the phenomenon of SLATY CLEAVAGE. We know that heat is developed by electricity when the free passage of electricity is IMPEDED; and we know, also, that this force is most powerful in dissolving and reconstructing the bonds of chemical union. May not the galvanic battery yet reveal the secrets of mineralogical affinities and segregation; as also of that ATOMICAL change which is probably the history of slaty cleavage, a change produced by thermo-electric currents acting upon stratified deposits, subjected for unnumbered ages to their influence? The slaty cleavage brigade would do well to arm themselves with the weapons of Volta ere they carry on another campaign against the rocks of Cambria.

PROCEEDINGS OF SOCIETIES.

British Association for the Advancement of Science, Dublin, August 26—September 2, 1857.

Continued from Vol. vi., p. 339.

SECTION E.—GEOGRAPHY AND ETHNOLOGY.

DR O'DONOVAN *on the Characteristics, Physical and Moral, of the Gaels of Ireland and Scotland.* He said—It is now universally admited by the learned that the *Gaedhil*, or ancient inhabitants of Ireland and of the Highlands of Scotland, and the Cymri, or Ancient Britons, are the descendants of the Celtæ of Gaul, and retain dialects flowing from the language of that people. The invariable tradition of the Gaedhil themselves is, that they came from Spain into Ireland. The earliest writer who mentions the Celtæ is Herodotus, who flourished about 413 years before Christ. He states that the Celtæ and Cymbræ dwelt in the remotest quarters of Europe, towards the setting sun; but the most copious and valuable account of them which has descended to us is contained in "Cæsar's Commentaries on the Gallic War." In this work they are described as a numerous and warlike people, who occupied nearly one-half of Gallia, or France. A colony of the same people occupied a great part of the north of Spain, where they were called *Celtiberi*, having crossed the Pyrenees from Gaul, and settled at first on the river Iberus, or Ebro, whence the name Celt Iberi. These, who were probably the ancestors of the Celtæ or Gaedhil of Ireland, are described as the most powerful and warlike of all the tribes or nations of Spain. Cæsar says that the people called Celtæ in their own language were styled *Galli* in the Roman or Latin tongue, but nothing is to be found in the "Commentaries" to throw any light upon this difference of name. The probability, however, is, that the Romans called them *Galli*, *i.e.*, "Cocks," from their pomposity and courage; though some think that Galli was but the Romanized pronunciation of Celtæ. At the present day the Welsh call the Irish and Highlanders *Guydhill*, but the two latter now style themselves *Gaoidhill*, or *Gaedhil*, aspirating the *dh*, as the English do there *gh*, although it is probable that the *dh* was pronounced originally. The identity of the race of the Celtæ of Gaul with that of the ancient inhabitants of Britain and Ireland may be argued from the same work, where it is stated that the great school of the Druids of Gaul was in Britain. The next authority relied on in proof of this identity is Tacitus, who, in his "Life of Agricola," states that there is very little difference between the soil and climate, the religious worship and dispositions of the inhabitants of Ireland and those of Britain. The paper then enumerates, after a German writer, a number of ancient Gaulish words that have been preserved by classic writers, and that afford strong grounds for believing that the language was a kindred one with the original dialects of the British Islands. The name of Celtæ was never applied to the Irish before the seventeenth century. They never applied it to themselves, but always understood it to be the name of the ancient inhabitants of France—"*Sco i sumas non Galli*," they said—"We are Gaels, not Gauls." It is clear that the Celtæ of Gaul had made considerable progress in civilization—that they had an order of priests called Druids, who believed in, and inculcated the doctrine of, the immortality of the soul and the metempsychosis—that they offered various sacrifices—that they worshipped Mercury as their favourite god, because they believed him to be the inventor of all the arts, and the promoter of mercantile affairs, and that next after Mercury they worshipped Apollo, Mars, Jupiter, and Minerva. Cæsar says, on the other hand, of the Germans, that they had no Druids to preside over religious affairs, and that they paid no attention to sacrifices, that they only worshipped those gods whom they saw with their own eyes—as the Sun, Vulcan (fire), and the Moon. In these passages the true line of distinction between the Teutonic and Celtic races is drawn by this great Roman general and statesman—a distinction which nearly holds good to the present day, after the lapse of nineteen centuries, and the various admixtures of the two races. Dr O'Donovan next proceeded to lay before the meeting certain facts regarding the ancient condition of the Gaedhil and Cymri, of which little notice had as yet been taken by any writers, introducing them by some remarkable instances of similarity between peculiarities recorded by Cæsar of the ancient Gauls and those of the Irish. Among these he alluded to the great stature of the Celtæ of Gaul and of the Gaedhil of Ireland, as compared with that of the Romans, and produced a number of authorities to show the stature, vigour, and valour of the ancient Irish. The paper next quotes Anglo-Norman and French testimonies of the stature and physical capabilities of the Irish in the reign of Richard II. (A.D. 1399), and in subsequent reigns, such as Froissart, Hollinshed, Spencer, &c., many of them exceedingly curious, from the quaintness of their style, as well as the statements which they contained.

Dr Wilde said—There were several modes by which we were enabled to arrive at an opinion as to the source of a people—the origin from which they spring. One of these modes was by the language of the people, another was history, and it was to this ground of opinion alone that his friend Dr O'Donovan had been referring; it was with that he dealt when he quoted Cambrensis on the stature of Dermet MacMurrogh; nor, in doing so, did he mean that that description of the King of Leinster was necessarily applicable to the people who inhabited Ireland a thousand years before. Another means of judging of ancient races was the monuments which they left behind; and a fourth was their own remains found in their tumuli, and accompanied by their weapons of war or implements of industry, or even objects of religion. These grounds of opinion had not been introduced by Dr O'Donovan; but as the results of the experience which he (Dr Wilde) had acquired in arranging the great Celtic Museum of their National Academy, the conclusion to which he had come was, that the first wave of population which visited this country was a very rude and simple race, which knew not the use of metals, and which formed its weapons by wearing two stones on each other. These were followed by another vital tide, composed of a totally different race, as the forms of their crania, discovered in the ancient tumuli, sufficiently demonstrated them to be. One of these races was long-headed, with low foreheads, high cheek bones, sunken eyes, and heads flattened on the sides. Some supposed these people to have been those called the Firbolgs. After these came a globular-headed race, with high foreheads, and every indication of a high degree of intellect; yet the remains of both of these races were found accompanied by the same kind of weapons, and in the same kind of tumuli or tombs. It remained to be seen whether future researches would confirm the theory on the subject; but it was right to know that those were the races which primarily constituted the people called the Gaedhil. He would take that opportunity to mention that he had been favoured with interesting specimens of these typical remains—namely, two skulls found in an ancient tumulus near Mullingar, which exhibited all the characteristics of the two races, and which, with the permission of their president, he would submit to their inspection on a future occasion.

Dr Norton Shaw, of the Royal Geographical Society of London, then proceeded to read *Captain Sherrard Osborne's paper on the Sea of Azof, and the Sivash, or Putrid Sea.* The rapid evaporation and the extraordinary mirage, from the heated atmosphere playing over the surface of this area in a summer's day, was very striking, and between sunrise and sunset at that season of the year it was as utterly impossible to distinguish objects but a mile or so distant upon it as it would be had a cauldron of boiling water been there in its place. The southern portion of the Sivash is about 40 miles long, commencing at the southern Chakrak, and ending at Fort Arabat. The Arabat spit throughout the whole of this distance is low and sandy, varying from 300 yards to 300 feet in width. Down the centre of the southern basin a maximum depth of about four feet six inches was found to exist, the water stealing away to either shore, until in calm weather 100 yards on each side was merely a quagmire, consisting of water, mud, decomposed vegetables, filth, and a foul, unctuous, bituminous deposit. * * Wherever the writer examined it, however, it was bitterly salt, and the hands tingled as if placed in strong brine. A most remarkable and general feature of the Sivash is the fluctuation of its depth according to the diversion of the wind. Next to these changes of level, and the rapid currents they occasion, the disagreeable exhalations from the shores of this sea have long been a subject of remark.

Dr Beddoe, *on the Physical Character of the Ancient and Modern Germans.* The subject of the paper was almost exclusively confined to the colour and complexion of the German races, as described by Tacitus, and as observed at the present day.

Mr Crawford, in making come comments on the paper, observed that, with reference to some of the preceding discussions on the Celtic races, it was curious that, in that Himalayan region, from which all the Celtic races were supposed to have come, not a single white person was now to be found.

Sir John Davis *on China, in more immediate reference to Pending Operations in that Quarter.* The paper, after some general remarks on the interest of the subject at the present moment, enters into a running but graphic description of the coasts of Canton river, Chusan, Shanghai, &c., showing the facilities which in many places they afford for defence, and for annoying the hostile fleet, but at the same time the facility with which any such annoyance on the part of the Chinese could be overcome.

Mr Gordon M. Hills, *on the Round Towers of Ireland*, detailed the results of a survey of those most interesting monuments which he had undertaken, and had already carried far towards completion. The drawings which he had made would, as already stated by the President, be exhibited at the Academy.

Mr Santiago Jackson *on Routes from Lima to the Navigable Branches of the Amazon, with Remarks on Eastern Peru as a Field for Emigration.*

The Rev. Dr Hinks read a paper *on the Ethnological inferences deducible from the Assyrian Modes of Writing.*

Herr Schlagintweit, *on the Route pursued by Himself and his Brothers in the Himalayas, Thibet, and Turkistan.* He said—that by the liberal arrangements made with himself and his brothers by the Right Hon. the Directors of the East India Company, they would be enabled at once to begin the publication of their researches in India. Their work would be entitled "Results of a Scientific Mission to India and High Asia during the years 1854, 1855, and 1856, by Herman, Adolphe, and Robert Schlagintweit." It would be accompanied by atlases containing topographical and geological maps of different parts of India, and of the Himalayas and mountains of Thibet. In 1854 he and his brothers Adolphe and Herman went from Bombay to Madras through Southern India, viz., the Deccan, Mysore, and the states of Nizam, on different routes, and met again all three in Madras. Already during their sea voyage they had occasion to make some observations on the temperature and specific gravity of sea water and the currents of the sea, and these observations they continued during their voyage from Madras to Calcutta. In the beginning of the year 1855 Herman Schlagintweit went through Bengal to Sikkim, and examined there the Himalayas, chiefly on the frontier between Sikkim and Nepaul. This route particularly afforded the advantage of enabling him to measure from a comparatively short distance the eastern Himalayas, including the peaks far to the eastward from Chamalari to a point considerably to the west of the high mountain of Nepaul. This was the same peak which had been recently measured by Colonel Waugh, who named it Mount Everest, after his distinguished predecessor. Its height was stated by that officer to exceed 26,000 feet. He and his brother obtained two names for it—one was Gororishanka, which was its sacred name, connected with Hindoo mythology, and only to be found in the Nepaulese holy books; the other was its Thibetan name, which was Chingofaumari. The name Deodunga, which was mentioned by Mr Hodgson in connection with this peak, was, as they were told in Katmando, the name of a low mountain, about 8000 or 9000 feet high, which was visible from the central parts of Nepaul, in about the same line as Mount Everest, and was remarkable for having upon its summit a sacred stone. After leaving Sikkim, H. Schlagintweit examined a part of the Bhootan Himalayas and Upper Assam, and returning to Calcutta, along the Brahmapootra and the delta of the Ganges, joined his brothers in Oude, in May 1856. He (Herr Robert Schlagintweit) and his brother Adolphe left Calcutta in March 1855, and passing through the north-west provinces, reached Naing Tal, whence they took different routes to Milum: from Milum they crossed over to Thibet, and in the disguise of merchants from Delhi, having merchandise but no money with them, succeeded in penetrating as far as the source of the Indus. Among the most interesting geographical features on this journey he might mention the following:—*First*, the alluvial deposits met north of the Himalayas formed by no means a plain, bordering the Himalayas to the northward, as the plain of Hindostan does to the southward. These alluvial and lacustrine deposits are merely filling up the irregularities in one of the greatest longitudinal valleys of the world. This valley included between the Himalayas to the south and the chief crest of the mountains of Thibet to the north, contains the course of the Indus to the west and the Dihong, the chief tributary of the Brahmapootra to the east. Both these rivers were separated only by a small rising of the surface of the valley. A well-known example of a similar formation in Europe was the form of the watershed between the Inn and the Dran-a-to-blach in the Tyrol. *Second*, North of the Indus a new high mountain range rose, covered with snow, and forming the watershed between Thibet and Eastern Turkistan. This range had been confounded with the Kuenheins, and its direction had never been properly defined, as it did not stretch from east to west. It was called in Ladak and Ballistan, the Karo-karum range, which signified black mountains—a characteristic name, for here the snow line was the highest in the world, being 18,600 feet above the level of the sea. From the peak of Goonashankur, 19,640 feet, they had the finest view which they had ever beheld, the course of which they could trace one hundred and fifty miles to the east of Gartok. They went from Gartok to examine the glaciers upon the high peak of Hi Ganwri, and having encamped on the 18th of August at the height of 19,220 at the top of the glacier, they succeeded, on the 19th of August, in attaining, on Ibi Ganuri a height of 22,260 English feet—the greatest height perhaps which, up to the present time, had been attained on any mountain. They returned by different routes. He himself crossed over a succession of passes into the valley of the Upper Ganges, where he examined a number of hot springs. In the cold season of 1855–6 they examined together the north-western provinces, and parts of Central India. Adolphe went to the south, along the course of the Godavery, and embarked for Madras. He (Herr Robert Schlagintweit) availed himself of the cold season to examine chiefly Central India up to the plateau of Amarkantak, the important watershed of Central India. Here he made some observations which, in a geographical point of view, were extremely interesting. The height of the plateau, which has never been measured before was 3300 feet; it was the culminating part of Central India, and the hills in its neighbourhood formed the watershed between four rivers, viz., the Nerbudda, the Soane, the Johilla, and the Mahammuddy. From Amarkantak he went to Simla, *via* Delhi. In the year 1856 Adolphe Schlagintweit left Simla for the Himalayas and Thibet on the 28th of May. On the 29th of July he reached an elevation of 19,500 feet on the Chorkonda Peak, on the mountains of Balkistan. On the 1st of September he arrived at the capital, and subsequently examined the group of mountains where the Indus makes its great bend to the south. One of these mountains, which reaches the height of 26,000 feet, is very remarkable on account of its position, which is at the end of the Western Himalayas. In the end of the year he proceeded to the Punjaub, where he made many geographical observations. At the western termination of the Himalayas, on the western side of the Indus, the range north of the Thibet and the Kuenhein, can no more be traced as separate chains, but form one mountain mass. Here they have lost to a great extent their alpine character, and no more large glaciers are to be met with. Western Thibet did not form a plateau, but was an undulating country, intersected by many high mountain ranges. One of the features of those parts, chiefly near the Moostak Pass, was the depression of the snow line here at an elevation of only 17,900 feet, which was perhaps owing to the great amount of snow and rain which fell in that country. At this time the heat of the deep valleys of Balkistan, which had an elevation of 7000 and 8000 feet, very little distant from the foot of the glaciers, was excessively high—the mean temperature from the 1st to the 20th July being 73 degs. to 75 degs. Fahrenheit, the minimum of the night being 59 degs. and the maximum being 90 degs. Herman Schlagintweit followed from Simla, first in a north-easterly direction, chiefly the line of the Thibetan Salt Lakes. Tsogan, Tsogar, Tsomikpat, and Tsomognalari, which was the greatest of all, and lay in Tsangkoog. These lakes showed a very different degree of concentration of the water. The lake Tsangkong had a specific quantity of 1003, and the water had no more a maximum of density above the freezing point, but contracted regularly till its freezing point at 31·5 degs. Fahrenheit. He reached Lehin Ladak, and rejoined his brother there. In the meantime he (Robert) travelled from Simla by Kooloo, Lahol, and Koopshoo to Ladak, passing over the Bara Lacha, Lacha Loong, and Trong Loong Passes. They went on from Leh on the 24th of July to Noobra, where, during a stay of three months, they reached the summit of Sassarda (about 20,000 feet). They then had to cross the plateau to the south of Karakorum, already visited by Dr Thompson, 17,100 feet high, and found afterwards much more extensive ones to the north of Karakorum Pass. They were nearly perfectly barren, and covered only with hills from 200 to 400 feet elevation, so small that they were enabled, for instance, to cross in one day from passes of more than 17,000 feet, which were but slightly elevated above the surrounding plateaus. After some further observations Herr Schlagintweit gave some details of researches in the territory of Nepaul.

Mr Beamish, F.R.S., *on the Human Hand, an Index of Mental Development.*

Dr Barth read a paper *on the Anomalous Period of Rising of the River Niger.* He said—Just at the present moment, when the route across the desert to the northern half of Central Africa seems to be cut off almost entirely, and when two more travellers by that route have fallen a sacrifice to their zeal, it may seem not quite unprofitable to consider the nature of that stupendous river, which, although not less ill-famed in consequence of the numerous sacrifices of valuable life which it has consumed, nevertheless affords a more easy access to the heart of the continent, and holds out the hope of a regular legitimate intercourse with the natives : for commercial intercourse, and exchange of produce and manufactures, and not conquest by way of arms, will be the means of bringing those vast and fertile tracts in contact with Europe. The Nile begins to rise in May or June, and to decrease in August or September, according to the more northerly or southerly position along its long course, and this is the general rule of all the rivers in the northern half of Central Africa, as well as in other parts of the world north of the equator; and it even obtains, with regard to the Benuwe, or the eastern branch of the Niger, where the waters at that point, where it is joined by the Faro, commence to decrease at the very end of September. But now the upper part of the Niger shows a quite anomalous state of things, for, during the month of August or September, the communication through the whole of the provinces along the Kwara is so difficult, on account of the numerous swollen rivulets and their swampy valleys, which do not in many cases admit of pontoons, and the climate is so unhealthy, that the travellers would certainly meet with a serious check in traversing the provinces of Nufe and Kebbi, during that season of the year. It is very remarkable, that in the lower part of the river, a second or later rising seems not to have been accurately observed and established, for there is no doubt that the phenomenon of which I am going to speak must exercise a full effect even upon that part of the river to which the name Kwara belongs. No doubt a large proportion of the aqueous element collected in the Upper Niger, or rather in that part which particularly deserves to be called Dhuliba or Juliba, the river of the Mandingoes or Juli, is lost by evaporation in the middle course between Sansanding and Timbuktu, where the river, called here Isa or Mayo, spreads out in a most extensive and marvellous net of backwaters, lakes, and creeks, affording free access to a vast area of fertile low grounds, and opening an immense line of inland navigation. In April it is possible to reach Kabara by water; the rising of the river continues without the slightest interruption till the month of February, filling out all the creeks, and inundating all the low grounds to a width of from twenty to thirty miles, and even closely approaching the very borders of the town, so that, from the beginning of January 1854, the smaller craft were able to approach within a few hundred yards of the great mosque which adorns the western end of the town of Timbuktu. Indeed, after having preserved the highest level for a fortnight or so, with an appearance of a little decrease now and then, followed by another rising of a few inches, a vacillation such as has been observed in other rivers, and is not but natural in sheets of water of such size, the inundation did not begin to diminish till the 17th of February, when the decrease became plainly visible, and continued without further interruption. It is this late rising of the river which gives to the climate of Timbuktu a very peculiar character, different, too, from other quarters of Negroland; and it follows that those very months, which all over India are the most healthy period of the year—December and January—constitute the most unhealthy season in Timbuktu,—a great deal of sickness prevailing there at that time of the year. The swelling of the river, and its following inundation, reached a rather unusual height in the season 1853–54. The river annually continues to rise till the end of January, and although it is true that it does not obtain such a height every year, nevertheless the inundation reaches the walls of Timbuktu almost regularly every third year. If we look for the probable reason of this unusual period of the rising of the river (disagreeing so completely with the period of swelling of the Nile), as well as of the other rivers in the northern half of Central Africa, including even the Benuwe, or the eastern branch of this very Niger, it is not easy to descover it. As for myself, I can only explain this phenomenon by a very heavy fall of rain, or a second rainy season in the month of November, in those quarters which lie at the back of Ashanti and the Gold Coast, and which are intersected by mountains of considerable elevation, but without being high enough to harbour even the smallest particle of snow, except perhaps for a couple of cold days in December or January. If we consider the long winding course of the river, the rain which falls in Kong and the other provinces of the Mandingoes or Wakore would naturally continue to swell the middle course of the river till the end of January, or even the middle of February.

Sir John Richardson's *Report of Mr Anderson's Search after the Crews of the Erebus and Terror.* Sir John Richardson believed it was quite certain that the boat—the fragments of which Mr Anderson had found on the shore of the lake Franklin—had been abandoned by Sir John Franklin's crew, and broken up by the Esquimaux; but he did not believe the story of the Esquimaux about the death of the crews from starvation at that particular place. He believed that the party had gone farther inland, and then died, and that their officers and best informed men had previously died, else they would have known of the depôt of provisions which lay to the north of where they then were, and which they did not unfortunately find out. When the expedition went out in 1845, they had provisions for 2½ years, or for 3 years, on short allowance; and in those regions of rigorous and perpetual cold, short allowance meant starvation. At the end of 3 years few of the men would have had strength enough left to enable them to travel far, and he had no doubt that they had all perished. If Sir James Ross had been able to penetrate by sledges to the point where it was intended that he should go, when first sent out on the search, he had no doubt that he would have found Sir John Franklin's ships, and if the new expedition arrive at that point, they most probably would find at least the remains of those unfortunate vessels.

Mr **Markham** *on the Search for Sir John Franklin, by M'Clintock's Expedition.*

Mr **Wm. Ogilby**, F.L.S., *on the Dispersion of particular kinds of Domestic Animals as connected with the great Ethnological Divisions of Mankind.*

Admiral **Fitzroy** *on the Possible Migrations and Variations of the Earlier Families of the Human Race.*

Papers were also communicated by Herr Hermann Schlagintweit *on some Measurements of different Races in India and the High Asia*, and by Professor W. R. Sullivan, *on the Influence which Physical Characteristics exercise upon the Language and Mythology of a People, as a means of tracing the Affinities of Races.*

Major-General **Chesney** read a paper *on our Communication with India by the line of the Euphrates and other Routes.* If a direct line be drawn along the globe from London to Bombay or Kurrachee, it exactly takes in the route by the valley of the Euphrates; consequently this portion of the line has necessarily formed a part of all the various projects that have been advanced with a view to facilitate and shorten our communication with India, with one exception, brought to my notice in a paper read last year at Cheltenham, which is supposed to go from Acre across the Desert to Bussorah. The distances by the two overland routes are as follows:—

	English miles.
From London to the entrance of the Red Sea, ...	4372½
From the entrance of the Red Sea to Kurrachee, which will no doubt become the great port of India in place of Bombay, ...	1705
Total, ...	6077½
London to the entrance of the Persian Gulf, ...	4271
From the entrance of the Persian Gulf to Kurrachee, ...	702
Total, ...	4973

—the difference in favour of the Euphrates valley being 1104½ miles. The great gain, therefore, is from the entrance of the Red Sea and Persian Gulf onward. From the Red Sea to Kurrachee we have 1705 English miles; whilst we have only 702 from the head of the Persian Gulf to the same port, or less than one-half. In the one case we have the monsoon right ahead towards Aden; in the other it is nearly abeam to Ormuz. A difficult and dangerous navigation in the one case, and a perfectly safe one in the other.

The Rev. Dr Hincks read a paper *on the Relation between the Newly-discovered Acadian Language and the Indo-European, Semitic, and Egyptian Languages; with Remarks on the Original Values of certain Semitic Letters, and upon the State of the Greek Alphabet at different periods.*

Mr George V. Du Noyer, M.R.I.A., *on the Remains of Early Stone-built Fortresses and Habitations in the County of Kerry.* He called attention to a class of Celtic antiquities hitherto but slightly noticed by archæologists, and especially to a Celtic city discovered by him in the summer of 1856. These buildings, which occupy an extent of three miles along the southern slope of Mount Eagle, consist of circular beehive-shaped houses, often surrounded by a massive circular wall, as if intended for warlike purposes; or of one, two, or three separate apartments, more or less circular in plan, and evidently intended for residences merely. Some are yet quite perfect, but generally the roofs have fallen in. One of the largest and most important of the former kind of building is called cahernamarturagh; and another called caheradurras, which is a triple-chambered building, offers a subject of study of the highest interest to the antiquary or inquirer into the architecture of the pre-historic inhabitants of Ireland.

Admiral Fitzroy said that it was curious that at the present time, in some countries in a partly savage state, the first kind of fortification erected by the natives during the time of which traces of their conduct could be obtained,—he alluded particularly to the New Zealanders,—corresponded exactly to the Irish fortifications, which might, he considered, be looked on not only as fortifications, but also as places of worship. The construction of the New Zealand places of defence consisted of an outer circle, in which the whole party and their cattle were protected, also a watch tower, and an inner citadel. There were also numerous passages from which sallies could be made. If wood were substituted for stone, the western Irish and New Zealand fortifications would be found almost identical.

The Rev. Dr Graves, F.T.C.D., remarked that the mode of architecture alluded to in the valuable paper read, should not be regarded as exclusively Irish, but as Celtic generally. He was of opinion that the stone caher were built by a race, kindred, though perhaps not identical, with the race that built Pictish towers in the north of Scotland.

Mr Babington mentioned that on the summit of a mountain in Carnarvonshire he had examined a building almost identical with those which were the subject of Mr Du Noyer's paper.

Mr John Hogg, F.R.S, read a paper *on the Supposed Biblical Names of Baalbec and on the Position of Baalgad.*

The Rev. Charles Russell, D.D., read a paper *on the Inhabitants and Dialect of the Barony of Forth, in the County of Wexford.*

Mr Kenneth Sutherland, R.N., *Observations on Vancouver's Island.*

Dr O'Donovan *on the Moral and Intellectual Characteristics of the Gael of Ireland and Scotland.*

Dr Seigfried *on an Inscription in the Language of Ancient Gaul, and on the recent Researches of Zeuss and others into that Language.*

Mr **Grattan** of Belfast, *on some Skulls discovered in an Ancient Sepulchral Mound near Mount Wilson, in the King's County.* He commenced by explaining a well-contrived and neatly-constructed apparatus for taking and recording cranial measurement, by which the exact value of any of the dimensions of the human skull can be taken and delineated with mathematical precision.

Dr **Humphrey Minchin**'s paper *on the Macrocephali* was then read.

Mr **Cull** read a paper *on the Ethnological value of the Indo-European Element in the Language of Finland.* He drew attention to the existtence of Indo-European words in the Finn language, and after showing the changes which took place in the forms of Swedish words on their incorporation into the Finn language, he proceeded to consider similar changes in Greek and Latin words. Accepting those already identified by Palmroth, Juslenius, Idman, Key, Wedgewood, and others, he extended the list very considerably, and pointed out Sanscrit words, which showed that words of all the Indo-European dialects are common to the Finn; and as these are words of daily life, he argued that the Finn language is Indo-European; and thence that all the Trisude dialects are so too, and thus he extends the term Indo-European to comprise the whole of the Turanian dialects; and as the Indo-European tongues have many roots in common with the Semitic tongues, he considers that the whole of the three families of tongues may be grouped as of one origin when the whole earth was of one speech.

Dr **Livingston** gave *a short Statement of Discoveries made by him in Southern Africa.*

Dr **Wilson**'s paper *on the Supposed Unity of the American Race*, and Mr **Crawford**'s *on the Affinities of the Hebrew and Celtic*, were read.

Professor **Antoine d'Abaddie**, the African traveller, gave some particulars as to *his Travels in Abyssinia.* He stated that the colour of the Negroes, in his opinion, was caused by the influence of a tropical sun, joined with vegetable diet. Before he went out he found from some books, and likewise learned from Dr Hodgson, that the eastern Negro had a large facial angle, and was much more intelligent than the western Negro. When he asked the natives of Ethiopia what a Negro was, they always said his skin is black, his heel protruding, he had a deep furrow near the top of his head, and his hair was no longer than his little finger. Both the Ethiopians and Negroes agreed in saying that their origin was from the east, that they crossed by the Isthmus of Bab-el-Mandeb—not the Straits of Bab-el-Mandeb, but the Isthmus—before their Hercules cut it through, as the Hercules of the Europeans did the Straits of Gibraltar. The Negroes admitted that the white men were their superiors. He had that opinion chiefly from children. But even the red men of Ethiopia admitted the superiority of the whites. Both Negroes and Red Ethiopians say that the Negro race resulted from the curse of God, and the direct interference of the Almighty. They say that all men were born white formerly, and they point out that they are still born white, even amongst themselves. He found two races living close by each other in the same climate, separated only by a few miles of mere ground—the Haggo and the Belaw race. The Belaws, according to their tradition, came from the Turks, and they still have Turkish names among them. Their appearance was European, but they lived on vegetable diet, and they were strictly black. They had even introduced Turkish into the Sematee language, which they used. The Haggo tribe eat milk and flesh, and there was not a black Haggo to be seen, and the same fact he had noticed on the southern borders of Ethiopia. He had known, on the upper table-land of Ethiopia, persons who had become black on being cured from a particular kind of disease, and he had seen them grow black beneath the eyes. There was something peculiar in the Ethiopian air, which sometimes thickened so much, even when there was no clouds, and when the atmosphere was perfectly dry, that a mountain some thousand feet high, could not be seen a few miles off. Such air, he thought, must have a peculiar influence on the skin, especially of a race living in it for many generations. He found that on the hands of the Negroes there was not the line which runs from the wrist to the middle finger, and he only found it amongst the nobility of the Galla tribes, who pretend that they are sprung from a white man, whom he supposed to be a Portuguese. None of this tribe could marry a relation nearer than the thirteenth degree. He found that these were the only race of men in the country stronger than himself; all the rest were weaker. Amongst the Danahil they always married their nearest kin, and they were each fallen away morally and physically. The Yeman sore, it was remarked, healed quite black in Africa, and remained black for months. These were the chief facts he had to commemorate; but the formation of races was a thing unknown. He found sometimes in the same family black, red, and almost white persons.

SECTION G.—MECHANICAL SCIENCE.

C. VIGNOLES, Esq., read a paper *on the Adaptation of Suspension Bridges to Sustain the Passage of Railway Trains.* The subject was comprised under the following heads:—1*st*, the maximum load to pass the bridge; 2*d*, the velocity of the train; 3*d*, the strength of the chain; 4*th*, the rigidity of the platform; 5*th*, prevention of undulation, vibration, and of oscillation. The novelty of the author's inquiry in the matters he adduced was confined to the question of the rigidity of the platform. He instanced the bridge over the River Dnieper, at Keiff, in Prussia, erected according to his designs, and stated that the successful resistance of the well-braced platform of this bridge to the effect of hurricanes and winds had been long remarkable. This bridge was completed about four years ago, just before the commencement of the Russian war, and at a time when he little thought the result of his exertions would be so soon used in facilitating military operations of the Russians against the allied forces. He alluded to the severe tests which it had successfully withstood in the conveyance of armies with heavy ordnance, and he came to the conclusion that the adaptation of suspension bridges to railway purposes is quite practicable; recommending, at the same time, that the speed of the trains, when passing, should be kept moderate, as compared with ordinary speed on railways.

The next paper read was by P. W. BARLOW, *on the Mechanical Effect of Combining Girders and Suspension Chains.* In this paper he described experiments which he had made, with a view of determining the applicability of the suspension principle, with a stiffened roadway, to a bridge proposed to be erected at Londonderry. He found that by a combination of a roadway of moderate stiffening with suspension chain, he was able to produce a bridge which, as a whole, was possessed of stiffness increased in a remarkable degree.

Mr Nasmyth made a report *On the Ventilation of Collieries*, and described a new and simple means of ventilating, which would have the effect of rendering explosion impossible by the instantaneous removal of the explosive gas. It consisted essentially of the placing at the head of the ventilating shaft a fan of enormous dimensions, not unlike those fans which are used in large factories as a substitute for bellows, worked by a steam-engine, the effect produced being the pumping out of the air from the shaft, which, of course, necessitates a corresponding current of air into the down shaft, and a rapid and continuous current through all the galleries, as is now attempted to be done ineffectually by placing a large fire at the exit shaft. One great advantage of this plan of Mr Nasmyth's is, that the amount of fuel necessary is about one-hundreth part of that used by the fire ventilators; there is no risk from explosion, as in the case of a fire, which is obviously exposed to such incidents, many of which he narrated; and in case of an accident from falling in, or otherwise, fresh air can be sent to the unfortunate sufferers below, because of the machine being placed at the top of, and altogether out of, the mine, and therefore beyond the reach of injury.

Mr J. Macgregor read a paper on *Early Modes of Propelling Ships.*

Mr J. Scott Russell proceeded to lay before the section some of the mechanical details of the construction of the great ship now building at his establishment at Millwall (the Great Eastern steamship). The first point related to the peculiarity of her great size; the second, on which her merits or demerits as a piece of naval architecture depended, was the general structure or lines of the ship; the third point would be the distribution of materials in the construction of the ship, so as to obtain the safest and strongest possible structure with the minimum of materials; and the last point would be the mechanical arrangements for her propulsion. In every case the smallest ship that would supply the convenience of trade was the right ship to build. The Great Eastern was the smallest ship capable of doing the work she was intended to do; and he believed that if she answered the purpose for which she was designed, she would continue to be the smallest ship possible for her voyage. It was found by experience that no steamship could be worked profitably which was of less size than a ton to a mile of the voyage she was to perform, carrying her own coal. Thus, a ship intended to ply between England and America would not pay permanently unless she were of 2500 or 3000 tons burden. In like manner, if a vessel were intended to go from this country to Australia or India without coaling on going out, but taking her coals with her, she would require to be 13,000 tons burden; and turning to the case before them, it would be found that the big ship was a little short of the proper size. Her voyage to Australia and back would be 25,000 miles; her tonnage, therefore, should be 25,000 tons, whereas its actual amount was 22,000 tons. The idea of making a ship large enough to carry her own coals for a voyage to Australia and back again was the idea of a man famous for large ideas—Mr Brunel. Wherever they found a steam-vessel with a high reputation for speed, economy of fuel, and good qualities at sea, he would undertake to say that they would find she was constructed on the wave principle. Now, the first thing to be done in building a steam-vessel was to make a calculation of the size of the mid-ship section in the water. In sailing from one place to another, it was necessary to excavate a canal out of the water large enough to allow the whole body of the ship to pass through. The problem was how to do that most economically, and this was effected by making the canal as narrow and as shallow as possible, so that there would be the smallest quantity of water possible to excavate. Therefore it was that the ship-builder endeavoured to obtain as small a mid-ship section as he could, and that had been effected in the case of the big ship, whose mid-ship section was small—not small absolutely, but small in proportion. In increasing the tonnage of a ship three things are to be considered—the paying power, the propelling power, and the dimensions. Mr Russell then entered into a calculation to show that while he doubled the money-earning power of a ship by increasing its size, he only increased its mid-ship section by 50 per cent. For instance, a ship of 2500 tons would have 500 feet of excavation through the water to do; the big ship had 2000 feet of excavation, and the lineal dimensions of the one were to the lineal dimensions of the other as 1 to 2·1. The excavation to be done by the big ship in relation to that to be done by the small ship was as 2000 to 500 feet, or four to one; but the carrying power was as 25,000 to 2500. To propel the big ship they had a nominal horse power of 2500, while to propel the smaller vessel there was a nominal horse power of 500; so that the big ship would be worked quite as economically as the small one. Referring, again, to the wave line, he would suppose that it was given as a problem to any one to design a ship on the wave principle. The first thing to be done was to settle the speed at which the ship was intended to go. If the speed were fixed at ten miles an hour, a reference to the table of the wave principle would show that in order to effect that object the length of the ship's bows ought to be about 60 feet, and of her stern about 40. If a larger vessel were required, say a ship of 130 feet long, there would be nothing more to do than to put a middle body of 30 feet in length between the bow and the stern. Having then made the width of the ship in accordance with the mid-ship section agreed upon, it would be necessary to draw what was known as the wave line on both sides of the bow, and the wave line of the second order on both sides of the stern. Constructed in this manner, and propelled by the ordinary amount of horse-power, the ship would sail precisely ten miles an hour. They could go slower than ten miles an hour if necessary, and in doing so they would economise fuel, in consequence of the diminished resistance of the water, whereas there would be a vastly increased resistance if an attempt were made to drive the steamer more than ten miles an hour. Now, with respect to the big ship. For the speed at which it was intended to drive the Great Eastern, it was found that the length of the bow should be 330 feet, the length of the stern 220 feet, of the middle body 120, and of the screw propeller 10 feet, making in all 680 feet in length. The lines on which she was constructed were neither more nor less than an extended copy of the lines of all ships which he had built since he first laid the wave principle before that Association. He would next refer to the mechanical construction of the ship, the arrangement of the iron of which she was made, and the objects of those arrangements. There was this great difference between the strength of iron and of wood, that, whilst the latter was weak crossways and strong lengthways, or with the grain of the timber, iron was almost equally strong either way. This had been clearly ascertained by experiments made by Mr Fairbairn and Mr M. Hodgkinson, at the request of the British Association, in whose Transactions the results were published to the world. The consequence was, that the ribs or frames used to strengthen wooden ships were rendered unnecessary in ship-building. Instead of the mass of wooden rubbish, which did not strengthen the ship, and involved enormous expense, he placed inside the iron shell as many complete bulkheads as the owner permitted him to do, and then constructed in the intermediate spaces partial bulkheads, or bulkheads in the centre of which holes had been cut for the purposes of stowage. The deck was strengthened by the introduction of pieces of angle iron and other contrivances; and, as an iron ship when weak was not weak crossways, but lengthways, he strengthened it in this direction by means of two longitudinal bulkheads, and the result was a strength and solidity which could not be obtained in any other way. The Great Eastern had all these improvements; and, in addition, the cellular system, so successfully applied in the Britannia Bridge, had been introduced all round the bottom and under the deck of the ship, giving the greatest amount of strength to resist crushing that could be procured.

On the Development of Heat by the Agitation of Water, by George Rennie, F.R.S., Vice-President.

M. Maya was the first who announced that heat was evolved from agitated water. The second was M. Joule, who, in 1842, announced that heat was evolved by water passing through narrow tubes, and by this method each degree of heat required for its evolution a mechanical force of 770 lbs. Subsequently, in 1845 and 1847, he arrived at a dynamical equivalent of 772 lbs. These experiments had since been confirmed by other philosophers, such as Seguin, Helmholtz, Frement, and Favre, on the Continent. In the present paper Mr Rennie stated that his attention was called to the subject by observing the evolution of heat by the sea in a storm, by the heat from water running in sluices. He therefore prepared an apparatus similar to a patent churn—somewhat similar to the churn adopted by M. Joule, but on a larger scale. In the first case he experimented on 50 gallons, or 500lbs. of water enclosed in a cubical box, and driven by a steam-engine, instead of a weight falling from a given height, as in M. Joule's experiment. Secondly, on a smaller scale, by 10lbs. of water enclosed in a box. The machine or churner in the large box was driven at a slow velocity of eighty-eight revolutions per minute, and the smaller machine at the rate of 232 revolutions per minute, so that the heat given off by the water in the large box was only at the rate of 3½° per hour, including the heat lost by radiation, whereas the heat evolved by the 10 gallons of water contained in the small box, agitated at 282 revolutions, was 56° of Fahrenheit. per hour. Thus, the temperature of the water in the large box was raised from 60° to 144°, and the temperature of the water in the small box to the boiling point. As an illustration, an egg was boiled hard in six minutes. The mechanical equivalent in the first case was found to approximate very nearly to that of M.

Joule, but in the latter case it was considerably above his equivalent, arising, very probably, from the difficulty of measuring accurately the retarding forces. Mr Rennie concluded his paper by remarking the great importance of the subject in mechanical science, and which had attracted the attention of the most eminent physical philosophers of the age, and particularly of the Royal Society of Berlin, which had offered a large prize for the accurate solution of the question.

Mr B. Stoney read a paper *on the Form of Entrances to Tidal Basins.* The chief points to be arrived at in constructing a dock or tidal basin were—1*st*, facility of ingress and egress; 2*d*, freedom from silting up. To these may be added, 3*d*, economy of quay room; and 4*th*, facilities for the land traffic in connexion with the shipping. These requisites were, he believed, in a great measure fulfilled in the form of basin and entrance which he now advocated, viz.,—a lozenge, a trapezium, or a rectangle, whose width was equal to the breadth of two vessels, together with sufficient space between them for another vessel to turn with facility, say from 350 to 400 feet between the walls for vessels of ordinary length. The entrance was at the lower end, and sloped, so that a ship could pass from the river into the dock without warping or any such annoyance and delay. Similarly, on leaving, a vessel, when once her head was round, could pass through with as much ease as at entrance, and without risk of being carried by the current against the lower pier-head. Every locality would, of course, require a distinct modification of the general principles described.

Mr Frith read a paper *on the Construction of Macadamized Roads.*

Mr G. Molesworth read a paper *on a Tangent-wheel as a Hydraulic Motor.*

Mr W. F. Dodds *on Improvements in Iron and Steel, and their Application to Railway and other Purposes.* By the improvements of the author, the furnaces for conversion are so constructed that they can be charged and discharged without reducing the temperature to any great extent, rendering it unnecessary for the men to enter the furnaces, excepting when stopped for the purpose of repairs. The malleable iron in the furnace is placed as in the ordinary method—the charcoal being mixed with a small percentage of lime and alkaline matter (limestone and soda ash by preference, being cheaper than any other), rendering the process of conversion more rapid—the heat in the furnace is not required nearly so strong as by the commom mode—and the time required for the complete conversion of the iron into steel is only from three to five days, being a saving of at least 60 per cent. in the amount of coal used in the process, and a saving in time of nearly fourteen days; so that one furnace, containing an equal amount of iron as an ordinary furnace, would be capable of converting from three to four times the quantity in the same time. The cost of the converting material is only 5 per cent. dearer than charcoal, and the quality of the steel made from any class of iron is much superior to the steel made from the same iron by the ordinary method. Again, the saving is further made apparent by the fact that less area of ground is required for doing a certain quantity of work, only one-third the number of furnaces being required, and the cost of the improved furnace being 20 per cent. less than those generally in use. The improvements also extend (by the use of the same furnace and converting materials) to the partial conversion of iron into steel, whereby the surfaces only are made into steel (which is applicable to numerous articles where the cost of steel precludes its use)—the depth of steel being generally governed by the length of time the iron or manufactured articles are left in the furnace to the action of the converting agent.

Mr A. HENDERSON read a report *on the Statistics of Gun-Boats.*

Mr ROBERT MALLET read a communication *on the Construction of the 36-inch Mortars* made by order of the Government during the late Russian war. The problem to be solved was the best mode of dealing with masses of earth and masonry in fortifications which were so large as to resist their ordinary ordnance. How could the range of projectiles be so increased that the ships employed in throwing could be comparatively out of reach of the guns of the fortress? He found that the largest shells used in modern warfare were 13-inch shells, weighing from 180 to 200 lbs., containing 9 lbs. of powder, and having a maximum range of 4700 yards. Such a shell penetrated about three feet into a mound of earth, but was incapable of piercing a wall of masonry twenty inches thick, save by reiterated blows. It occurred to him that if they could project a shell of much greater weight, containing more powder, and with an enormously increased projectile power, they would be able to effect the demolition of fortifications with greater ease and rapidity. He therefore proposed a shell of three feet in diameter, which would contain about 500 lbs. of powder, and have an increased range. This would give an enormously increased power of demolition. Such shells entering into the midst of a fortification would act like a mine or series of mines, exploding with great force, and causing an immense destruction of masonry. He calculated that a shell of that description would have a penetrative power into compacted earth of fifteen feet, and that the range might be probably increased by as much as twice that of the 13-inch shell, proving an instrument of destruction immeasurably superior to anything which they already possessed. It appeared a necessary condition to this problem that the mortar should be constructed in several pieces, because so large an instrument could not be forged without sustaining flaws in the process of cooling.

Mr JAMES OLDHAM read the continuation of a report read in 1853, *on the Rise and Progress and Present Position of Steam Navigation in Hull.*

Mr T. SILVER, U.S., *on the Importance of Regulating the Speed of Marine Engines.* There was exhibited a model governor, constructed upon a new principle, and possessing this advantage over others, that it was not in the least affected by the pitching of the vessel.

Mr JAMES BARTON proceeded to describe *the Principles upon which the Boyne Viaduct had been constructed*, and, in explanation of his statement, referred to a large model of the viaduct. The dimensions of the work were—height above high-water mark, 90 feet; opening of the centre span, 204 feet; and of the two side spans, 140 feet each.

Professor THOMSON made a statement *on Machinery for Laying Submarine Telegraph Cables.*

Mr ANDREW S. HART, F.T.C.D., read a paper *on the Effect of the Resistance of the Water to an Extended Cable.*

Mr H. **Wright** communicated a paper by Palestrini *on his Submarine Electric Telegraph Cable.*

Mr J. S. **Ward** communicated a paper from J. Brackenridge, *on the Working and Ventilation of Coal Mines.*

Sir **James Murray** read a paper *on the Laying of Submarine Telegraph Cables.*

The Chairman remarked that, assuming the cable to be of the right description, he believed there was sufficient mechanical talent in this country and in America to construct such machinery as would pay out the cable safely.

Mr **Neville** read a communication *with reference to the Flow of Water through Circular Pipes.*

Mr B. A. **Murray** made some observations *on the Spinning of Silks from the Cocoon*, and exhibited a model of the machinery by which the new process was effected. He said that silk spun in this manner was perfectly free from knots, and, consequently much superior to the article produced by the old system; in addition it caused a considerable saving of labour.

Mr J. **Hayes** made some observations *on the Mode of Rendering Peat Economically Available as a Fuel, and as a Source of Illuminating Gas.*

Mr **Thomas Moy** read a communication *on Improvements in the Working of Steam-Engines, and on the Philosophy of the Wave-Line System.*

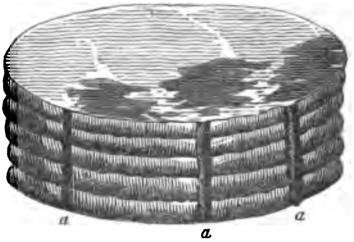
Dr **Gray** *on a New Railway Signal,* which had been tested very satisfactorily upon the Midland Great Western Railway.

Proceedings of the American Association for the Advancement of Science.

On the Varieties and Mode of Preservation of the Fossils known as Sternbergiæ, by J. W. **Dawson**, F.G.S.—The fossils which have been named Sternbergiæ, and sometimes Artisiæ, are usually mere casts in clay or sand, having a transversely wrinkled surface, and sometimes an external coaly coating, and traces of internal coaly partitions. They are found in the coal formation rocks of most countries, and very abundantly in those of Nova Scotia. Until the recent discoveries of Corda and Williamson, they were objects of curious and varied conjecture to geologists and botanists, and were supposed to indicate some very extraordinary and anomalous vegetable structure. They are now known to be casts of the piths or internal medullary cavities of trees, and the genera to which some of them belong have been pointed out. Many interesting truths with respect to them, both in their geological and botanical relations, still, however, remain to be developed; and in the present paper I propose to offer some further contributions toward their history, and the geological inferences deducible from it.

In a paper communicated to the Geological Society of London, in 1846, to which Professor Williamson, in his able memoir in the Manchester Transactions,* assigns the credit of first suggesting that connection between these curious fossils and the conifers, which he has so successfully worked out, I stated my belief that those specimens of Sternbergiæ which occur with only thin smooth coatings of coal, might have belonged to rush-like endogens; while those to which fragments of fossil wood were attached, presented structures resembling those of conifers. These last were not, however, so well preserved as to justify me in speaking very positively as to their coniferous affinities. They were also comparatively rare; and I was unable to understand how casts of the pith of conifers could assume the appearance of the naked or thinly-coated Sternbergiæ. Additional specimens, affording well-preserved coniferous tissue, have removed these doubts, and, in connection with others in a less perfect state of preservation, have enabled me more fully to comprehend the homologies of this curious structure, and the manner in which specimens of it have been preserved independently of the wood.

Fig. 1.



Portion of Sternbergia (nat. size), (*a*) Remains of woody fibre.

My most perfect specimen is one from the coal-field of Pictou (fig. 1). It is cylindrical, but somewhat flattened, being 1$\frac{2}{10}$ inch in its least diameter, and 1$\frac{7}{10}$ inch in its greatest. The diaphragms or transverse partitions appear to have been continuous, though now somewhat broken. They are rather less than $\frac{1}{10}$th of an inch apart, and are more regular than is usual in these fossils. The outer surface of the pith, except where covered by the remains of the wood, is marked by strong wrinkles, corresponding to the diaphragms. The little transverse ridges are in part coated with a smooth tissue similar to that of the diaphragms, and of nearly the same thickness.

When traced around the circumference, or toward the centre, the partitions sometimes coalesce and become double, and there is a tendency to the alternation of wider and narrower wrinkles on the surface. In these characters, and in its general external aspect, the specimen perfectly resembles many of the ordinary naked Sternbergiæ.

On microscopic examination the partitions are found to consist of condensed pith, which, from the compression of the cells, must have been of a firm bark-like texture in the recent plant. The wood attached to the surface, which consists of merely a few small splinters, is distinctly coniferous, with two and three rows of discs on the cell walls. It is not distinguishable from that of *Pinites* (*Dadoxylon*) *Brandlingi*, of Witham, or from that of the specimens figured by Professor Williamson. The wood and transverse partitions are perfectly silicified, and of a dark-brown colour. The partitions are coated with small colourless crystals of quartz and a little iron pyrites, and the remaining spaces are filled with crystalline laminæ of sulphate of barytes.

Unfortunately this fine specimen does not possess enough of its woody tissue to show the dimensions or age of the trunk or branch which contained this enormous pith. It proves, however, that the pith itself has not been merely dried and cracked transversely by the elongation of the stem, as appears to be the case in the Butternut (*Juglans cinerea*), and some other modern trees; but that it has been condensed into a firm epidermis-like coating and partitions, apparently less destructible than the woody tissue which invested them. In this specimen the process of condensation has been carried much farther than in that described by Professor Williamson, in which a portion of the unaltered pith remained between the Sternbergia-cast and the wood. It thus more fully explains the possibility of the preservation of such hollow-chambered piths, after the disappearance of the wood. It also shows that the coaly coating investing such detached pith-casts is not the medullary sheath, properly so called, but the outer part of the condensed pith itself.

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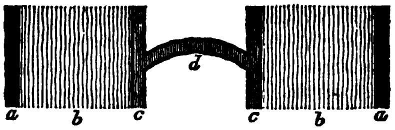
* Vol. ix., 1851.

The examination of this specimen having convinced me that the structure of Sternbergiæ implies something more than the transverse cracking observed in Juglandaceæ, I proceeded to compare it with other piths, and especially with that of *Cecropia peltata*, a West Indian tree, of the natural family Artocarpaceæ, a specimen of which was kindly presented to me by Professor Balfour of Edinburgh, and which I believe has been noticed by Dr Fleming, in a paper to which I have not had access.* This recent stem is two inches in diameter. Its medullary cylinder is $\frac{3}{4}$ths of an inch in diameter, and is lined throughout by a coating of dense whitish pith tissue, $\frac{1}{20}$th of an inch in thickness. This condensed pith is of a firm corky texture, and forms a sort of internal bark lining the medullary cavity. Within this the stem is hollow, but is crossed by arched partitions, convex upward, and distant from each other from $\frac{3}{4}$ths to $1\frac{1}{4}$th inch. These partitions are of the same white corky tissue with the pith lining the cavity; and on their surfaces, as well as on that of the latter, are small patches of brownish large-celled pith, being the remains of that which has disappeared from the intervening spaces. Each partition corresponds with the upper margin of one of the large triangular leaf scars, arranged in quincuncial order on the surface of the stem. (fig. 2.)

Fig. 2.



Longitudinal section of recent *Cecropia peltata* (nat. size). (*a*) Bark; (*b*) Wood; (*c*) Pith lining medullary cavity; (*d*) Diaphragm of pith.

Inferring from these appearances that this plant contains two distinct kinds of pith tissue, differing in duration, and probably in function, I obtained, for comparison, specimens of living plants of this and allied families. In some of these, and especially in a species labelled *Ficus imperialis* from Jamaica, I found the same structure; and in the young branches, before the central part of the pith was broken up, it was evident that the tissue was of two distinct kinds—one forming the outer coating and transverse partitions opposite the insertions of the leaves, and retaining its vitality for several years at least; the other occupying the intervening spaces or internodes, of looser texture, speedily drying up, and ultimately disappearing.

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* Dr Fleming's remarks occur in the Proceedings of the Botanical Society of Edinburgh.—Edit. *Phil. Jour.*

Another variety of the Sternbergia-like pith structure appears in a rapidly growing exogenous tree with opposite leaves, cultivated here, and I believe a species of Paullinia. In this trunk there are thick nodal partitions, and the intervening spaces are hollow, and lined with firm corky pith, with its superficial portion condensed into a sort of epidermis, and marked with transverse wrinkles; a cast of which would resemble those Sternbergiæ which have merely wrinkles without diaphragms.

The trunks above noticed are of rapid growth, and have large leaves; and it is probable that the more permanent pith tissue of the medullary lining and partitions serves to equalize the distribution of the juices of the stem, which might otherwise be endangered by the tearing of the ordinary pith in the rapid elongation of the internodes. A similar structure has evidently existed in the coal formation conifers of the genus Dadoxylon, and possibly they also were of rapid growth, and furnished with very large or abundant leaves.

I have no means of ascertaining to what extent this structure may characterize certain botanical families, nor what gradations it may present between the mere transverse cracking observed in the trunks of the Butternut and other Juglandaceæ, and the perfect partitions developed in *Cecropia.* Professor Gray states that the transverse pith structure is characteristic of the North American trees of the genus *Juglans* but wanting in the closely-allied genus *Carya*—a parallel case with its apparent restriction to one genus, or perhaps species, of extinct conifers. It is quite possible that some of the more rapidly growing and thicker-branched species of southern conifers still present similar structures. The axes of cones also deserve study in this respect, since I have observed that the pith of the cone of *Pinus Strobus* shows, though obscurely, a tendency to the formation of transverse dissepiments.

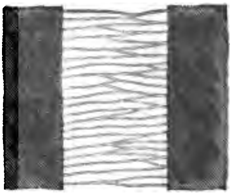
Applying the facts above stated to the different varieties or species of Sternbergia, we must, in the first place, connect with these fossils such plants as the *Pinites medullaris* of Witham. I have not seen a longitudinal section of this fossil, but should expect it to present a transverse structure of the Sternbergia type. The first specimen described by Professor Williamson represents a second variety, in which the transverse structure is developed in the central part of the pith, but not at the sides. In my Pictou specimen the pith has wholly disappeared, with the exception of the denser outer coating and transverse plates. All these are distinctly coniferous, and the differences that appear may be due merely to age, or more or less rapid growth.

Other specimens of Sternbergia want the internal partitions, which may, however, have been removed by decay; and these often retain very imperfect traces, or none, of the investing wood. In the case of those which retain any portion of the wood, sufficient to render probable their coniferous character, the surface-markings are similar in character to those of my Pictou specimen, but often vary greatly in their dimensions, some having fine transverse wrinkles, others having these wide and coarse. Of those specimens which retain no wood, but only a thin coaly investment representing the outer pith, many cannot be distinguished by their superficial markings from those that are known to be coniferous, and they occasionally afford evidence that we must not attach too much importance to the character of their markings. A very instructive specimen of this kind from Ohio, with which I have been favoured from Professor Newberry, has in a portion of its thicker end very fine transverse wrinkles, and in the remainder of the specimen much coarser wrinkles. This difference marks, perhaps, the various rates of growth in successive seasons, or the change of the character of the pith in older portions of the stem.

I have not been so fortunate as to find any of the Sternbergia or Artisia casts associated with the wood of plants allied to Lepidodendron, as observed by M. Corda. There are, however, in the collection of Professor Newberry, as well as in my own, specimens which present very considerable differences in their external characters from those of the varieties known to have been coniferous, and which may be the axes of such plants.

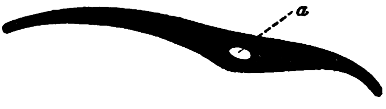
The state of preservation of the Sternbergia casts in reference to the woody matter which surrounded them, presents, in a geological point of view, many interesting features. Professor Williamson's specimen I suppose to be unique in its showing all the tissues of the branch or trunk in a good state of preservation. More frequently, only fragments of the wood remain, in such a condition as to evidence an advanced state of decay; while the bark-like medullary lining remains. In other specimens the coaly coating investing the cast sends forth flat expansions on either side, as if the Sternbergia had been the midrib of a long, thick leaf. This appearance, at one time very perplexing to me, I suppose to result from the entire removal of the wood by decay, and the flattening of the bark, so that a perfectly flattened specimen, like that in fig. 3 may be all that remains of a coniferous branch nearly two inches in diameter. A still greater amount of decay of woody tissue is evidenced by those Sternbergia casts which are thinly coated with structureless coal. These must, in many cases, represent trunks and branches which have lost their bark and wood by decay; while the tough, cork-like chambered pith drifted away to be imbedded in a separate state. This might readily happen with the pith of *Cercopia*; and perhaps that of these coniferous trees may have been more durable; while the wood, like the sap-wood of many modern pines, may have been susceptible of rapid decay, and liable, when exposed to alternate moisture and dryness, to break up into those rectangular blocks, which are seen in the decaying trunks of modern conifers, and are so abundantly scattered over the surfaces of coal, and its associated beds, in the form of mineral charcoal.

Fig. 3.



Flattened Sternbergia, with compressed bark (nat. size).

Fig. 4.



Flattened trunk, one foot in diameter, with Sternbergia. (*a*) Portion of Sternbergia cast.

Some specimens of Sternbergia appear to show that they have existed in the interior of trunks of considerable size. The best instance of this that I have found is that presented in fig. 4, from the South Joggins, and which appears to show the remains of a tree a foot in diameter, now flattened and converted into coal, but retaining a distinct cast of a wrinkled Sternbergia pith.

Are we to infer from these facts that the wood or the trees of the genus Dadoxylon was necessarily of a lax and perishable texture? Its structure, and the occurrence of the heart-wood of huge trunks of similar character in a perfectly mineralized condition, would lead to a different conclusion; and I suspect that we should rather regard the mode of occurrence of Sternbergia as a caution against the too general inference from the state of preservation of trees of the coal formation, that their tissues were very destructible, and that the beds of coal must consist of such perishable materials. The coniferous character of the Sternbergiæ, in connection with their state of preservation, seems to strengthen a conclusion, at which I have been arriving from microscopic and field examinations of the coal and carbonaceous shales, that the thickest beds of coal, at least in Eastern America, consist in great part of the flattened bark of coniferous, sigillaroid and lepidodendroid trees, the wood of which has perished by slow decay, or appears only in the state of fragments and films of mineral charcoal. This is a view, however, on which I do not now wish to insist, until 1 have further opportunities of comfirming it by observation.

The most abundant locality of Sternbergia with which I am acquainted, occurs in the neighbourhood of the town of Pictou, immediately below the bed of erect calamites described in the Journal of the Geological Society (vol. vii., p. 194). The fossils are found in interrupted beds of very coarse sandstone, with calcareous concretions, imbedded in a thick reddish-brown sandstone. These gray patches are full of well-preserved calamites, which have either grown upon them, or have been drifted in clumps with their roots entire. The appearances suggest the idea of patches of gray sand rising from a bottom of red mud, with clumps of growing calamites, which arrested quantities of drift plants, consisting principally of Sternbergia and fragments of much decayed wood and bark, now in the state of coaly matter too much penetrated by iron pyrites to show its structure distinctly. We thus, probably, have the fresh-growing calamites entombed along with the debris of the old decaying conifers of some neighbouring shore; furnishing an illustration of the truth that the most ephemeral and perishable forms may be fossilized and preserved, contemporaneously with the decay of the most durable tissues. The rush of a single summer may be preserved with its minutest striæ unharmed, when the giant pine of centuries has crumbled into mould. It is so now, and it was so equally in the carboniferous period.

Proceedings of the Royal Institution of Cornwall.

Remarkable Ancient British Caves near Penzance. By R. EDWARDS, Junior.—The following is the most remarkable of all ancient British caves hitherto discovered in Cornwall.

Half of a mile W.S.W. of Caër Brân, and four and a-half miles W. by S. of Penzance, there is, in the village of Chapel Euny, a cave, consisting for the most part of a deep trench, walled with stones, and roofed with huge slabs. It extends 30 feet from N.N.W. to S.S.E., and then branches eastward, and probably also to the S. or S.W. So far it accords with the description of an ordinary British cave. But its floor (as I was informed by the miner who opened it about three years ago) was well paved with large granite blocks, beneath which, in the centre, ran a narrow gutter or bolt, made, I imagine, for admitting the external air into the innermost part of the building, from whence, after flowing back through the cave, it escaped by the cave's mouth—a mode of ventilation practised immemorially by the miners in this neighbourhood, when driving adits or horizontal galleries under ground.

Another peculiarity is still more remarkable. Its higher or northern end consisted of a circular floor, 12 feet in diameter, covered with a dome of granite, two-thirds of which are still exposed to view; and my informant had observed a still greater portion of the dome-roofed chamber. Every successive layer of the stones forming the dome overhangs considerably the layer immediately beneath it; so that the stones gradually approach each other as they rise, until the topstones must originally have completed the dome; not, however, like the key-stones of an arch, but by resting horizontally on the immediately subjacent circular layer. These topstones, and probably the layer next under them, had all fallen into the cave before the miner opened it; some being so large that he could not remove them until he had broken them by blasting. He found no pottery, nor anything else in the cave. The height of the present wall of the dome is about 6 feet above the lowest part I could see; how much lower the original floor might have been, I could not ascertain. The cave, although partially opened, would still occupy a labourer some days before the stones and rubbish could be removed for its complete examination. This is probably the cave referred to by the late Rev. John Buller fifteen years ago, in his account of St Just (p. 82), but which then had "not been examined."

Another British cave, not even referred to in any publication, is to be seen at Chyoster, nearly three miles north of Penzance, the walls of which, instead of being perpendicular, are constructed on the same principle as the inmost part of the cave at Chapel Euny, so that the tops of these walls, which support the huge slabs forming the roof, are much nearer each other than their bases. This I observed at the higher or northern end of the cave, which remains undisturbed; whereas the walls and roof of the lower part, to the extent of several yards, have been removed. The higher part of this excavation has not yet been completely explored; and it possibly may contain a dome-roofed chamber like that now described. Each cave formed part of a British village, that of old Chyoster being decidedly in the best state of preservation of all the British villages in this neighbourhood.

It is due to the Cambrian Archæological Association to mention, that what I have above communicated will appear in their next quarterly Journal as part of my paper on British Villages, being the fourth of a series, which I am writing at their request, on the Celtic Antiquities of the Lands and District. (Read 30*th October* 1857.)

Note on Subterranean Temperature observed in Chili. By William Jory Henwood, F.R.S., F.G.S., Member of the Geological Society of France, &c. &c.—The mountain of Chanarcillo, about fifty miles from the Pacific, and the same distance south-east of Copiapo, the chief city in Atacama, the northern province of Chili, rises about 4000 feet above the sea, and rather more than 2000 above the undulating plain which isolates it from the lower Andes. It presents a steep escarpment towards the north-east, but on the south-west it slopes gradually to the plain. In this declivity the rich and well-known silver-mines of the district are wrought. The position of the mines presents every facility for natural drainage; whilst the less than half a dozen showers—the only fall of rain during the year—scarcely penetrate more than a few inches into the parched soil. No water, therefore, occurs in any of the mines; and the surface is destitute alike of springs and of vegetation.

The rocks of which the mountain is composed consist of three beds of grayish limestone (destitute, so far as I observed, of organic remains), —which alternate with two of hornblendic rock, which, for brevity sake, may be called greenstone. All of them are equally traversed by three separate and distinct series of *lodes*, which suffer neither interruption of course, alteration of dip or dimensions, or indeed any change, except of course in mineral composition, on passing from one rock into another. The chief, or most productive of them, are the *Colorada*, which bears about 20° E. of N., and W. of S., and dips towards the N.W.; the *Descubridora*, which bears about N. and S., and dips towards the W.; and the *Candelaria*, which bears about 35° N. of E., and S. of W., and dips towards the N. or N.W. On the course of each of these lodes several mines in succession have been opened; and each of them is occasionally accompanied by branches, which sometimes reunite with the parent vein, and sometimes dwindle, and ultimately die away in the (country) adjoining rock. It is in the limestones alone that these lodes and branches are metalliferous; in the greenstones they are all alike unproductive. Several cross-veins traverse, and usually heave the lodes, but the strata are displaced in a single instance only, in the mine of San Francisco Viejo.

Having lately visited Chili professionally, I embraced the opportunity thus afforded me to make a few observations on subterranean temperature, —a subject which has not, I believe, hitherto attracted attention there. The deepest mine below the surface of the district is the *Colorada;* of which the mouth is 3656 feet above the sea, and about 1750 above the surrounding low ground; its depth is 1500 feet (about 250 fathoms), its deepest point, therefore, has not yet reached down to the level of the plain. My observations were made in holes, which, by the kindness of

Edwin Price Waring, Esq., the resident superintendent, had been bored about two feet into the rock near the *lodes*, some days before my visit, to give time for the dissipation of any heat which might have been generated by the boring; they were, however, carefully plugged during the interval, to avoid, as far as possible, any influence from the air circulating through the mine, which is admirably ventilated, even to the bottom. As my stay in Chili was limited to a few weeks during the depth of the southern winter, I do not know the range of temperature throughout the year at the surface;* nor could I ascertain at what depth in the earth climatic changes cease to be appreciable.

Depth from Surface.	Part of the Mine.	Temperature of Rock in a hole two feet deep.	Temperature of air, circulating through mine, same locality.
		° ′	° ′
48 fathoms.	First limestone rock, between Waring's lode E. and Colorada lode W. . .	64 8	66 0
127 „	Second limestone rock, between Waring's lode E. and Colorada lode W. .	67 5	66 76
150 „	Second limestone rock, at the bottom of shaft, . .	67 (†)	65 0
227 „	Third limestone rock, W. wall of Colorada lode. Unfrequented part of mine, N.	72 0	76 0
227 „	Third limestone rock, E. wall of Colorada lode. Frequented part of mine, S.	74 5	76 5

Although these results exhibit undoubted evidence that the temperature

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* Temperature of air in the shade, at the surface of the Colorada mine 3656 feet above the Pacific;—

1857.	7 A.M.	9 A.M.	Noon.	3 P.M.	6 P.M.	9 P.M.
June 9	58°5	62°8	...	...	...	...
„ 10	...	61°	66°5	61°6	56°5	50°5
„ 11	...	46°8	53°8	...	...	48°8
„ 12	44°	45°	...	...	...	...
„ 13	39°5	43°8	...	...	44°	42°
„ 14	39°8	42°	48°	46°	42°	41°8
„ 15	42°8	43°8	...	...	...	...
Means,	44°9	49°3	56°1	53°8	47°5	45°8

If in the absence of observations at midnight and at 3 A.M., we assume the mean temperatures at those hours to be 45°, which at this period cannot be very wide of the truth, we have an average of about 48°5 during the 24 hours.

We cannot fail to notice the conspicuous change of climate which took place on the 11th of June; on that day a thermometer exposed to the sun at noon stood at 66°8.

† This observation, made at the bottom of the shaft, where the draught is of uncommon velocity, ought, perhaps, to be excluded from the general average.

increases as we descend, they also present anomalies difficult, if not impossible, to reconcile. They may, however; possibly be due to some disturbance of the natural equilibrium between the respective temperatures of the rocks which form the walls, and of the air circulating through the interior of the mine; dependent, perhaps, on its greater or less velocity, which may make each in turn the vehicle and the recipient of heat. The ratio of progressive increase in temperature is much slower in the case before us than in the schistose, or even in the granitic rocks of Cornwall and Devon)* a fact of which, as far as relates to the stratified rocks of some other countries, I became long since aware, from a comparison of my own observations here, with those of others elsewhere; but whether it has been already published I do not, at present, recollect; nor can I at this moment refer to the details on which that conclusion was grounded. A much wider field of research must, however, be examined before we can venture to pronounce on its generality. What function, if any, in the economy of nature, may be connected with the contact of two series of rocks, in which the progressive elevation of temperature is so different for equal increments of depth as it is in the granite and slate rocks of this country; or with—as in the order of geological sequence—the interposition of a formation in which that progression is a rapid one, between two others—differing as well from it as from each other in structure and composition†—in which the ratio is much slower, is perhaps an obscure and a difficult inquiry; but it must be an interesting, and may be an important one.

German Scientific Meeting at Bonn in 1857.

The thirty-third annual meeting of German Naturalists and Physicians was held this year in Bonn, and having had an opportunity of witnessing a portion of the proceedings, it has occurred to me that a short account of what came under my notice may possess some interest for at least a portion of your readers.

You are aware that it is to these meetings that the plan of the British Association owes its origin. The late Professor Oken is the man to whom the Germans are indebted for their first organization, and he himself received his idea from Switzerland. In noticing the proceedings of the Swiss naturalists in his Isis, Oken frequently took occasion to represent the advantages which Germany might derive from similar reunions, where the members, becoming personally acquainted, could interchange their opinions, communicate and endeavour to resolve each other's doubts, and afford each other mutual encouragement in the path of scientific inquiry. The first meeting took place at Leipzig in 1822, but it was several years before the number of participators rose so high as thirty. The stream, however, if not broad, was deep from the outset. Gradually it became wider. The meeting just closed, though by no means so numerously attended as that held last year at Vienna, mustered to the number of nine hundred and sixty, and included many of the most eminent names of Europe in the various departments of science. In the geological section, of which I formed an unworthy member, I observed Merian, Rose, Von Carnall, Blum, Noeggerath, Murchison, Elie de Beaumont.

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* Cornwall Geological Transactions, vol. v. 387. Edinburgh New Philosophical Journal, 1843.

† As the slate series does from the granite below, and from the interstratified limestones and greenstones above; and as they do from each other.

The proceedings of the first general meeting were opened on 18th September by Professor Noeggerath, who greeted the assembly with genuine German *bonhomme*. His appearance reminded me of a weather-beaten column of basalt, which seemed to bid eternal defiance alike to time and to tempest. Dr Kilian then read various letters of compliment or apology, the most interesting of which was a note from Alexander von Humboldt, who had been specially invited to assist at the proceedings, but excused himself on the ground of the necessity he felt himself to be under at his advanced period of life to employ every available moment of his time in the completion of the works which he had now in progress. On Professor Noeggerath's motion, the whole assembly rose up, with acclamation, to testify their respect for the illustrious veteran; and a telegraphic message was despatched to him on the instant, informing him of this grateful tribute of homage. Humboldt's acknowledgment, I may here add, was received a few hours afterwards.

After the proceedings had been duly opened, Professor Shultz-Shultzenstein delivered an address on the value of the natural sciences as a means of educating the human mind. Professor Mädler of Dorpat then read a contribution on the subject of the fixed stars. The motions, he said, of certain fixed stars were not compatible with the assumption of a central sun; nor did the assumption of partial systems appear admissible, inasmuch as, for the explanation of the size of the measured motions of individual fixed stars, the central masses—if such existed—must possess a mass incredibly great. The centre of gravity of the fixed sidereal system, which may possibly lie in empty space, was to be regarded as the centre of motion. If the system possessed a globular form, with a nearly uniform distribution of the masses in the interior of the globe, the period of revolution of the various masses would be of nearly similar length, so that the whole, viewed from one of the stars in conjunct motion, must appear nearly immovable. A more definite decision was to be expected only from later centuries enriched with the spoils of long series of observations. The speaker considered it probable that the central point lay in the region of Taurus, perhaps in the group of the Pleiades, the apparent motions of which seemed best to harmonise with that assumption.

Dr Hamel, of St Petersburg, then delivered a discourse, in which he endeavoured to trace the history of the invention of the Electric Telegraph. The first telegraphic apparatus worked by galvanism was that exhibited by Soemmering on the 29th August 1809, before the Academy of Sciences at Munich, in which the mode of signalling consisted in the development of gas-bubbles from water placed in a series of glass tubes, each of which denoted a letter of the alphabet. Baron Schilling, attached to the Russian embassy at Munich, was a particular friend of Soemmering's, and a frequent visitor at his laboratory in 1807 and 1808, when he was occupied with his galvanic telegraph. When Oersted, in 1820, published his important discovery, it occurred to Schilling that the instant declination of the magnetic needle on the application of a stream of galvanism through a surrounding wire might be applied to telegraphic purposes; and although Ampère, no doubt, so early as the autumn of 1820, had announced an application of Oersted's discovery to telegraphy, as something that was perhaps possible, Schilling was the first to realize the idea by *actually producing* an electro-magnetic telegraph, simpler in construction than that which Ampère had *imagined.* By degrees he succeeded in producing an apparatus with which, by means of a wire several (German) miles long, he was able successfully to transmit electro-magnetic signals, previously sounding an alarm when required. His journey to Mongolia (commenced in May 1830) interrupted for a time his telegraphic labours, but he speedily resumed them upon his return home in 1832. The services of Professor Weber of Göttingen in the same cause in 1833, Dr Hamel passed over, as already known to his auditory. In May 1835 Baron Schilling left St Petersburg on a tour through Germany, France, and Holland, and he attended the meeting of German Naturalists which took place that year in Bonn. At the sitting of the Physical Section on the 23d September, of which the President for the day was Professor Muncke of Heidelberg, Schilling exhibited and explained his telegraphic apparatus, with which Muncke was greatly taken. He frequently spoke of it after his return to Heidelberg, and on the 6th March following (1836) he explained the whole thing to William Fothergill Cooke, who was then occupied at the Anatomical Museum, with Professor Tiedemann's sanction, in the preparation of wax models for his father, then recently appointed Professor of Anatomy in the University of Durham. Cooke, although he had never previously studied physics or electricity, was so struck with what Muncke told him, that he instantly resolved on abandoniug the work he was engaged on, and on endeavouring to introduce electro-magnetic telegraphs upon the English railways. With this object in view he reached London on the 22d April. On the 27th February 1837 he became acquainted with Professor Wheatstone of King's College; and early in May the two gentlemen resolved to labour in common for the introduction of the telegraph into England—an object which they successfully accomplished. On the 12th June they obtained their patent, and on the 25th July the first trial was made at the London terminus of the North-Western Railway with a wire a mile and a-quarter long. About a fortnight previously, Steinheil of Munich had placed the buildings of the Academy of Sciences in electric communication with the Observatory at Bogenhausen; and his discovery, the following year, of the possibility of bringing the galvanic current, in telegraphing through the earth, back to the battery, deserves greater recognition than it has yet received.

Schilling, on his return to St Petersburg, had renewed his efforts to turn the telegraph to useful account with more energy than ever. After a serious of experiments, he believed he had succeeded in effecting a sufficient isolation of the conducting wire to admit of the transmission of signals through water, and he proposed to unite Cronstadt with St Petersburg by means of a submarine cable. He had got a rope prepared with several copper wires isolated agreeably to his instructions, when death put a stop to his labours on the 7th August 1837.

In the course of the summer of that year intelligence reached America of what had been done in Germany and England in the way of electric telegraphy. This news stimulated Samuel F. B. Morse to construct, with the assistance of Dr Dale, Professor of Chemistry, an apparatus with which he hoped to be able to telegraph. The subject was not at that time quite new to Morse. He had been twice over in Europe to improve himself in his profession as a painter; and in the course of his second homeward voyage in 1832, he had had his attention awakened to the possibility of electro-magnetic telegraphy by Dr Jackson, his fellow-passenger on board the Sully. On the 4th September—a month after Schilling's death—he made what he termed a "successful attempt." The speaker was in possession of a sketch prepared by Morse himself of the apparatus with which this successful attempt was effected. By means of a set of flat-toothed types there was impressed upon a sheet of paper, moved horizontally over a cylinder, a set of zigzag marks like the teeth of a saw, which were meant to denote figures. In this manner a set of numbers was presented to the eye, each denoting a certain word or number, for the ascertainment of which the receiver of the despatch required to consult a voluminous dictionary. The stripe of paper operated upon on the 4th September 1837, represented, in teeth shaped somewhat like the letter V, the following numbers, viz.:—215, 36, 2, 58, 112, 04, 01837, which, according to the dictionary, denoted "Successful experiment with telegraph, September 4, 1837." This cumbrous process, of course, never came into actual use; but, notwithstanding this, Morse boldly terms himself the inventor of electric telegraphy, and dates his invention from the year 1832. Nay, more, the Supreme Court of the United States pronounced a judgment in 1854, finding that in this respect he had the priority of all Europe. It may possibly be worth while to observe that Morse is not, as seems to be commonly supposed, a Professor of Physics. In 1835 he was appointed "Professor of the Literature of the Arts of Design" in the educational institution termed the University of New York; but he never delivered a single lecture. The instrument now known by the name of Morse's Telegraph was brought to perfection by degrees, long subsequently to 1837, and after Morse had made two more voyages to Europe.

In November 1839, Cooke and Wheatstone executed in London a contract of copartnery, and on the 12th December they gave in their specification. Their process was founded essentially on the same principle as Schilling's, only giving the needle a vertical, instead of a horizontal position. In August 1839 there were completed thirteen miles—namely, from Paddington to Drayton—of a telegraphic line along the Great Western Railway, then in progress. Other extensions followed; and in 1845 Cooke received commissions for a number of lines in various directions throughout the country. The telegraph had received a sudden accession of popularity from the aid it had afforded in the discovery and apprehension of John Tawel the murderer. In 1846 Cooke succeeded in forming the Electric Telegraph Company, which afterwards amalgamated with the International. Their head station is at Lothbury, and down to the present day most of the apparatus employed by them are constructed on the principle originally applied by Schilling, though now greatly improved by Wheatstone. From these apparatus proceed 150 different wires at the least, which run below the pavement to various localities.

Thus it was Baron Schilling of Lanstadt who was the first man by whom electro-magnetic telegraphy was really applied; and it was the telegraphic seed from St Petersburg which, after finding its way *vià* Bonn and Heidelberg to England, struck its roots in London—roots from which a tree has sprung up whose gigantic branches, laden with golden fruit, now stretch and ramify over land and sea.

After the delivery of Dr Hamel's address, and a few words upon the subject of it from Colonel von Siebold and Dr Drescher, the meeting separated into the various sections, where the only business performed was the election of their respective presidents.

On Saturday, September 19th, the proceedings of the Geological Section commenced with some observations by Dr Jäger of Stuttgart, on the origin of regular forms in rocks, which he referred to processes of crystallization in the sedimentary masses. Dr Otto Volger of Frankfort exhibited a series of specimens with the view of demonstrating the results of his inquiries (already published) on the history of the development of mineral bodies, and the mode in which the various rocks originate. Dr Pichler of Innsbruck exhibited a geognostic map of the northern limestone Alps of the Tyrol, from the borders of the Vorarlberg to the borders of Salzburg, and spoke at some length upon the different formations. Dr Von Dechen gave information with respect to the geognostical map of Rhenish Westphalia, of which eleven sections had already appeared, and nine others were in course of preparation. Professor Plieninger spoke upon the difference in the formation of the teeth between the *Microlestes antiquus*, from the upper breccia (betwixt the Keuper and the lias) of Wurtemberg, and the *Plagiaulax* of the Purbeck oolite. Herr von dem Borne discoursed on the geology of Pomerania, referring to the alluvium, the diluvium, the tertiary strata, and the Jura formations. The alluvium is found chiefly on the sandy coasts, greatly changed by currents. It is washed away from the Pomeranian, and deposited on the Prussian coast. In the diluvium he distinguished a disturbed recent formation and a regularly deposited older one.

On Monday (21st September) I happened to take a look in at the Physiological Section. Professor Mayer was just taking the chair, and thanking the meeting for the honour of his election. He then referred in a feeling manner to the deep loss which science and the medical art had recently sustained in the too early death of Dr Marshall Hall, who, however his claims may have been contested, was undoubtedly the author of the theory of the reflex function of the spinal marrow. Dr Mayer proceeded to notice the merits of the deceased in regard to the physiology of the nerves and the doctrine of asphyxia. In reference to the latter subject, he laid before the meeting a little work which he had received only a few weeks ago from the deceased, on the method of restoring persons apparently drowned, and concluded by calling on the meeting, in recognition of the deceased's services to medical science and suffering humanity, to rise from their seats and honour his memory with a *sit ei terra levis.* The manner in which the President's proposal was instantly responded to showed the high estimation in which our countryman was held by his brethren in Germany.

On getting into the Geological section I found Gustav Rose making some observations on the gneiss which forms the north-western limit of the granitite of the Riesengebirge, and of the granite which occurs in it: he also spoke of the relation of granite to gneiss in general. The boundaries betwixt the two could, he said, be very distinctly drawn in the Riesengebirge. Last year at Vienna the learned Professor had given an account of some recent investigations which he had made in the Riesengebirge and Isergebirge, with a view to determine the exact limits betwixt granitite and granite, and assigned the reasons which had induced him to regard the former as a separate species of rock from the latter. These reasons were:—first, the distinct mineral composition—the white mica of the granite being entirely wanting; secondly, the accurate limits which can be drawn betwixt it and the granite of the Isergebirge; and, thirdly, the circumstance that a mixture of similar composition to the granitite of the Riesengebirge and Isergebirge occurred in the most diverse localities. From the relations of the granitite to the granite, the Professor considered that the former must have penetrated to the surface more recently than the latter. [See also a contribution by Rose, "Ueber die zur granitgruppe gehörigen Gebirgsarten" in the first volume of the "Zeitschrift der Deutsch-geologischen Gesellschaft."]

Sir Roderick Murchison laid before the meeting the most recent publications of the Geological Survey, consisting of maps, sections, &c., as illustrative of the Silurian or older palæozoic rocks, the coal measures, and the secondary and tertiary deposits; and he also referred to the records of the School of Mines and the Decades of Organic Remains, which exhibited the labours of various distinguished English geologists. M. E. de Verneuil observed that, whilst Sir R. Murchison had borne such willing testimony to the distinguished merits of his colleagues, he had entirely overlooked his own services; and pointed out that, in regard especially to the School of Mines, Sir Roderick had had the greatest share in its extension and results, both through the great works which he had himself accomplished, and through what others had accomplished under his guidance and superintendence.

Her Von Carnall exhibited a copy of the new edition of his geognostical map of Upper Silesia, and explained in what respects it differed from the first edition. He took occasion to remark that of the ironstone rocks of Upper Silesia, it was only a portion that could be regarded as middle Jurassic; the portions of this formation lying to the north and west of Oppeln, and the Rybnik and Rattibor portions must be regarded as tertiary-miocene. Under these strata lay the Upper Silesian gypsum and marl rocks (tegel), with traces of salt, which were now in the course of being investigated.

Professor Von Zepharovich (of Cracow), spoke of the progress that had recently been made in the knowledge of Austrian minerals, and pointed out the necessity of collecting and arranging the results of inquiries made during long periods of time in order to obtain a synoptical view of what had really been accomplished. He then exhibited a few printed sheets of a large work of this description applicable to the Austrian empire, and mentioned that the work itself would probably be published in the course of next year. He then handed to the President a piece of fossil iron from Chotzen in Bohemia. Thereupon Dr O. Volger, with reference to the aqueous origin of iron, mentioned the fact, that Herr

Von Baer had found, in a fossil tree imbedded in the turf of a floating island on the coast of Sweden, which only occasionally emerged from the water, that the mass by which the cells had been replaced consisted of native iron.

The proceedings of the day were concluded by a few short but exceedingly interesting remarks from Professor Blum (Heidelberg) on the causes of the formation of different combinations of crystals in the same species of mineral. On this subject, he observed our knowledge was exceedingly scanty. We had scarcely a single observation or inquiry to which we were able to refer. Experiment alone presented us with facts by the aid of which we might possibly make some progress. It was a familiar fact, that when an easily soluble salt (alum) crystallized from a pure solution, the forms exhibited differed from those which were obtained from impure solutions. This fact was sufficient of itself to show beyond a doubt that *the medium* in which substances crystallize exerts an influence upon the form of the crystal. Taking this for our principle, and applying it to nature, we find it to be a fact that certain minerals, when they occur in certain rocks, appear under one and the same form of crystal—when magnetic iron ore, for example, occurred in chlorite schist, it was found in general to occur in the form of an octahedron. The subject was worthy of careful investigation, and might turn out to be of very great importance in a geognostic point of view.

At the sitting of Tuesday, September 22, Professor Daubrée of Strasburg spoke on the formation of sulphuret of copper and apophyllite from the thermal springs of Plombières. In the course of certain excavations, undertaken with the purpose of fencing in these springs, the speaker had found two recent substances, which were of geological interest, from the resemblance they bore to certain minerals. On a bronze cock, of Roman workmanship, which had been lying amidst the rubbish of ancient buildings for more than fifteen centuries, sulphuret of copper had been formed in the shape of beautiful crystals. They belonged to the hexagonal system, and could not be distinguished from natural crystals. From a similar composition, artificial crystals belonging to the regular system had already been obtained. The circumstances under which they had been formed seemed to differ from those under which the formation of similar crystals occurred in veins. The ancient mortar into which the warm water percolates includes in its cavities small colourless crystals identical in form and composition with apophyllite. They owe their formation to the operation of the silicate of potash from the hot springs on the lime of the mortar. The formation both of the apophyllite and of the hexagonal sulphuret of copper had here taken place in water of which the temperature did not exceed 70° C.

Dr Volger spoke on the subject of earthquakes, and particularly the earthquake in the Valais in the year 1855. The cause of it he referred not so much to Volcanic action as to aqueous erosion, whereby the superior strata had lost the stratum on which they had rested.

Dr Abich spoke on the subject of mud volcanoes, and their importance in geology. He founded this importance on an analysis of the history of the development of these formations as they occur in the environs of the Caucasus, particularly in the two Caucasian peninsulas, Taman and Apsoheron, and endeavoured to establish the following propositions:—1.

The stratographic facts of the before-named localities afford a proof that the structure of these formations, notwithstanding the Neptunian origin of the masses of which they are composed, is determined by precisely the same laws which regulate the various forms of mountains composed of strictly Vulcanic masses that have arisen in the mode of igneous fluidity. 2. The distribution of those small independent systems of mountains is most distinctly subordinate to the grand lines which determine the direction of mountain ranges, and therewith the fundamental features of our continents. 3. The linear grouping and serial arrangement of these mountains in accordance with these lines of elevation, was regulated by the same laws which regulated the foundation and successive completion of the mountain systems and ranges of every portion of the earth's surface. In conformity with these principles, Dr Abich maintained that every view was to be rejected which might incline to refer the eruptive phenomena which still retain their permanent seat in the bosom of these formations to so-called secondary causes, that is, in the present case, to any other causes than such as depend upon Vulcanism.

Herr J. Beissel spoke on the marl of Aix-la-Chapelle, and laid before the section a geological collection from the Friedrichsberg, and the Willkommsberg, in the neighbourhood of that city. The distinction hitherto assumed between the Aix and Bohemian chalk on the one hand, and the Westphalian on the other, grounded on the occurrence of polythalamiæ and cirrhipeds in the former, must now be done away with. Ehrenberg's discovery that marl consists of organic bodies is confirmed. The greensand has arisen from a marly rock by the loss of its carbonate of lime. Down to the present time the marl is passing into sand-beds under the influence of fresh water. The proofs which he adduced were:—1. Those fossils which characterize the greensand are found in banks of sandstone which have lost every particle of lime, in banks of sandstone containing lime, in the banks of Dumont's psammite glauconifère. 2. The speaker had himself found the characteristic fossils of the upper beds of the Aachan chalk in dry deposits of Greenland. 3. The glauconite granule is in most cases the result of the formation of a stone nucleus in the shells of polythalamiæ. 4. On dissolving the marl in muriatic acid we obtain a residuum of greensand. That the lower portions of the chalk are precisely those which have lost their lime is explained by the circumstance that, being the last to be elevated above the sea, they were the longest exposed to the influence of the sea water; moreover the meteoric waters flow over the clay strata of the Aachan sand, and thus fill the lower division whilst they merely filter through the upper. The speaker then discussed the residuum of the marl and sandstone:—1. The double-refracting siliceous splinter; 2. The single-refracting glauconite granules; 3. The double-refracting spongiolites. The siliceous splinters originate:—1. From spongiolites which become crystalline on the change of the amorphous silica; 2. From the disintegration of the white stone granules of polythalamiæ; 3. From glauconite granules which have burst and lost their colouring matter, and of which the amorphous silica had been changed into crystalline. The speaker's collections, and especially his microscopic preparations, of the finest organisms, excited in the section the utmost admiration.

At the sitting of Wednesday, September 23, General von Panhuys explained a small geological map of the southern portion of the Duchy of Limburg, which he had prepared in 1850, by instructions of the Dutch War Office. The object had been to ascertain whether the coal measures extended to the Dutch territory. The speaker endeavoured to show that the Bardenberg district, north of Aix-la-Chapelle, is connected with the Liege coal trough, and forms a portion of it. Were this the case—a fact that can be perfectly ascertained only by borings—Limburg would be in possession of two square miles of coal measures, of which one-half is covered merely by greensand, and the other half by greensand and chalk.

Herr Von der Marck spoke on the subject of some petrifactions of the Westphalian chalk, and exhibited a number of new and well-preserved fossils—amongst others, the remains of huge saurians from the Schöppinger Berg, near Münster.

Herr Heymann spoke of the changes of certain constituents that had occurred in trachytic and basaltic rocks in the Siebengebirge. He exhibited specimens of oligoklas transmuted into kaolin and red Ehrenbergite; of hornblende transmuted into steatite; of transmuted augite and olivine in the basalt of the Menzenberg, near Honnef; radiated mesotype from the basalt of the Minderberg, and also partly changed into a steatitic mass.

Professor Noeggerath denied that the black mica in the trachytes was altered hornblende.

Her Max Brann observed that the occurrence of blende at the Wetternsee in Sweden was something very different from what it is in our known veins and beds in the district of the Rhine. In Sweden the blende-formed beds which were imbedded in the gneiss, following the gneiss strata, with similar strike and dip, for a considerable extent, and with a thickness of 15 to 20 feet or more. The blende is for the most part finely granular, and always intimately mixed with more or less feldspath. In these beds of blende are found concretions of green feldspath and of quartz, including crystalline particles of blende. The gneiss in immediate contact with the blende contains a bed of granular lime, containing garnet and pistazite and thin layers of Wollastonite. Parallel to the blende strata is a bed of brown garnet, containing mica and dichlorite, and in like manner subordinate to the gneiss. There were similar layers of white cobalt and copper pyrites imbedded in quartzose mica-slate. This occurrence of zinc blende is peculiar, and does not seem to harmonise well with our common views regarding mineral veins.

Sir Roderick Murchison exhibited the plates of a new edition of his Siluria, and explained the most important additions that had been made to our knowledge of the Silurian rocks during the last three years. He maintained that it was now proved, both by physical and by zoological facts, that the Bala beds of Wales were identical with the Caradoc beds, resting similarly upon the Llandeilo formation, in the lower division of which a number of new fossil species had been discovered. He then referred to the group of the Llandovery rocks in South Wales (containing the *Pentamerus oblongus*) lying between the lower and upper Silurian, and closely connected with each. Finally, he exhibited figures of gigantic crustaceans (Pterygotus) found in the upper Silurian beds, which had been published by Mr Salter in the Decades of the Geological Survey.

M. Ch. St Claire Deville exhibited his topographical map of the island of Guadaloupe. In the centre rises the cone of the Sonfriere, surrounded by a crater of elevation. The latter consists of dolerite; the central cone of a trachyte, the feldspath of which approaches in chemical composition to Labrador. The Sonfriere is an extinct volcano. At the request of Sir Roderick Murchison and Mr Merian, the speaker then communicated his views with regard to the volcanoes of Italy and their mode of action. He held by Von Buch's theory of elevation, but laid considerable stress upon *étoilement*. Vesuvius and Etna, as central volcanoes, he regarded as the points of intersection of radiating fissures, in which volcanic action bursts forth. The Phlegnean fields, the Rotta Monfina, the Lago d'Amsanto, Ischia, and other points, he considered as lying upon these fissures.

Herr von Carnal exhibited maps of the coal formation in Russian Poland on a scale of $\frac{1}{20,000}$, and of Lower Silesia, at which Beyrich, Rose, and Roth, had been working for years, on a scale of $\frac{1}{100,000}$.

Director Nanck, with reference to the question agitated on Monday by Professor Blum, reported the result of a series of experiments undertaken with a view to the arbitrary production of secondary surfaces on artificial crystals. He described the method employed by him, by means of which he found that the number of surfaces became greater in proportion to the slowness with which crystallization proceeded, a fact of which he cited several examples. He stated, in conclusion, that his experiments should be continued.

Professor Römer communicated the result of a survey of the Jurassic Wesergebirge between Hameln and Osnabrück. He referred especially to the striking alterations which the members of the Jura formation composing the range undergo in the course of their extent. In consequence of such a change, for example, the Oxford appears in the western spurs of the chain as compact quartz, whilst in a section of the Porto Guestphalica it is developed in layers of loose sandy marl-schist, which crumbles to pieces in the atmosphere. As something altogether peculiar to the Wesergebirge, and differing from anything to be found either in other parts of North Germany or in any other district, he denoted the occurrence of thick beds of brown sandstone in the uppermost member of the series, which is distinguished chiefly by *Exogyra virgula*, the member which in North Germany has hitherto been denoted as Portland, but would more properly be termed Kimmeridge. Such sandstone strata may be observed in the neighbourhood of Lübeck and of Preussisch Oldendorff.—*Correspondent of Scotsman.*

SCIENTIFIC INTELLIGENCE.

BOTANY.

The Climate and Cultivated Plants of Norway.—The comparative mean temperature of Christiania and of the few places on the coast under the moderating influence of the sea, and especially of the Gulf Stream, where any meteorological observations have been made, give the following results:—

	Mean Annual Temperature.	Mean of Three Coldest Winter Months.	Mean of Three Hottest Summer Months.
Christiania, lat. 59°·54,	41°·675 F.	23° F.	59°·9 F.
Ullensvang in Hardanger, lat. 60°, ...	45°	33°·8	60°·125
Drontheim, lat. 63° 30′, .	39°·65	27°	59°
North Cape, lat. 71°, .	32°·225	23°	43°·25

The result of this extraordinary winter mildness is, that the sea never freezes on the whole of the western and northern coasts. Wheat is cultivated up to Inderoen, lat. 64°; oats to Salten, lat. 68° 30′; rye, both as winter and spring corn, to Dyro, lat. 69°, and has yielded even twenty-two fold at Hassel, lat. 68° 30′; barley ripens at Alten, lat. 70°, but one degree from the North Cape; potatoes succeeded well at Vadrö, in the Russian frontier, rather above 70°; and turnips are there also very generally cultivated.

These results are facilitated by the great rapidity of summer growth, evidently influenced by the long duration of light at these high latitudes. At Alten, lat. 70°, the sun remains above the horizon from the 24th May to the 10th July. Barley, which on account of the night frosts (by daylight), cannot be sown before about the 20th to the 24th June, is often reaped before the end of August, yielding six to seven-fold. Mr Thomas, an Englishman settled there for many years, and now a member of the Norwegian Diet, has observed that barley will grow 2½ inches in 24 hours, and pease full 3 inches.

Owing to the abrupt termination towards the sea to the westward of the great mountain mass of Norway, the fall of rain on the western sea-coast presents a striking contrast to that of the western. Whilst at Christiania the average annual amount of rain for the last twelve years is under 20 inches, it amounts at Bergen to about 80 inches. The summer heat is, however, so much moderated along the western coasts, that it sometimes happens that in the islands and along the shores of the mainland the corn has to be cut green, when further to the north it has ripened well inland. The extremes of heat and cold in the interior are sometimes considerable. At Valle in the Sœtersthal, lat. 59° 15′, at an elevation of 1100 to 1200 feet, and far from the coast, the maximum summer heat reaches 108°·5 F., and the minimum winter cold descends to – 31° F.

The following are the fruit trees and shrubs of Norway as enumerated by Mr Schuebeler:—

The apple is wild, and grows as far north as 63° 30′. The pear is not wild,—cultivated as far north as the apple. Quince and medlar and mulberries in gardens about Christiania. Cherry succeeds as far north as 66° 15′. Plum will succeed as far north as 63°. Peaches on espalier ripen in South Norway. Apricots are grown on espalier up to 63° of lat. Walnuts ripen up to lat. 61°. *Juglans nigra* ripens its fruit at Christiania. Hazel-nut will ripen its fruit up to 66° lat.; in lat. 63° it thrives up to an elevation of 1000 feet. Chestnuts in favourable summers ripen their fruit in lat. 58° to 59°. Almonds also occasionally ripen fruit near Cape

Lindernœa. Grape vine, cultivated on espalier, will ripen on the Sogne-fiord; it is usually covered in winter at Christiania. Elder in warm summers ripens fruit up to 63° 30′ lat., and so also the barberry. Red currant ripens up to 70°; black currant to about 63°. Gooseberries are wild in South Norway; varieties are cultivated up to 60° 15′. Raspberries are wild up to 70° lat. *Rubus arcticus*, rare in South Norway; it is common in northern parts, and in favourable summers ripens its fruit up to 70° lat. *Rubus Chamœmorus* extends to the North Cape. The strawberry, *Vaccinium Myrtillus*, *V. uliginosus*, *V. Vitis-Idæa*, and *V. Oxycoccus*, are diffused abundantly over the whole of Norway. *Rosa canina* bears fruit up to lat. 66°; *R. cinnamomea* up to lat. 70°. *R. villosa* up to 65° or 66°; while *R. rubiginosa* is found only on the south coast.—*Schuebeler on the Geographical Distribution of Fruit Trees and Berry-bearing Shrubs in Norway, as translated in Gardeners' Chronicle.*

Properties of Urticaceæ.—Urtication or blistering with nettles, according to Weddell, is still used both by civilized and savage nations, in cases where sudden irritation is required. *Laportea gigas* is the tree nettle of New South Wales, which is famous for its blistering qualities. The most important fibre-producing species are *Urtica dioica*, *U. cannabina*, *U. parviflora*, *Bœhmeria nivea*, *Maoutea Puya*, *Laportea canadensis*, *Girardinia heterophylla*, *Pipturus propinquus*. In the cells of the epidermis of Urticaceæ there exist concretions of carbonate of lime called cystoliths, which are suspended by stalks of cellulose.

CHEMISTRY.

Constitution of the Fatty Acids, Aromatic Acids, Aldehydes, Acetones, &c., and their Relations to Carbonic Acid.[*] By Professor KOLBE.—In 1848, Professor Kopp put forward the view that formic and acetic acid, as well as the fat acids generally, contained conjugate radicals, having two atoms of carbon as a common constituent, together with one atom of hydrogen, methyl, ethyl, or some other of the ethylic radicals, associated with C^2. He also considered cacodyl to be such a conjugate radical, consisting of one atom arsenic, together with two atoms of methyl $(C^2 H^3) As_2$; the methyl and ethyl-dithionic acids to contain conjugate sulphur radicals $(C^2 H^3) S^2$ and $(C^4 H^5) S^2$, and that other elements, such as selenium, phosphorus, and antimony might be capable of forming similar conjugate radicals.

This latter conjecture has since been confirmed by the discovery of methyl, and ethyl-zinc, ethyl-tin, &c. by Professor Frankland; methyl-antimony, by Professor Löwig, and ethyl-tellurium and methyl-selenium by Professor Wöhler. He considers that the mode in which these substances are produced, their direct formation from their constituents, by, simple or double decomposition, especially indicates that they are conjugate radicals. If it were possible to produce in a similar manner, by the action of iodide of methyl upon carburet of iron $(F C^2)$, a radical, having the composition $(C^2 H^3) C^2$, which would combine directly with oxygen, producing acetic acid, there would be no doubt that this radical was acetyl, and that it should be classed with methyl-mercury, ethyl-tin, and similar substances.

Meanwhile Professor Frankland has put forward an idea, which Professor Kopp considers is likely to be productive of good results, if further followed out, and which would modify the views entertained as to the constitution of the fat acids, their analogues, aldehydes, acetones, &c. Professor Frankland has observed that the organic conjugate radicals, containing metals always evince a smaller capacity of saturation for oxygen, chlorine, and similar elements, than the corresponding metals themselves in their normal state. In this respect there is definite uniformity observable, which is indicated by the following facts.

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[*] Annalen der Chemie und Pharmacie, vol. ci. 267.

Every metal—and probably every element—that is capable of combining with hydrogen, methyl, ethyl, &c., so as to produce conjugate radicals, not only acquires thereby a more strongly-marked positive character than it possesses in the normal state, but this also increases with each additional atom of hydrogen, methyl, &c., which enters into the conjugate radical.

The number of atoms of a negative element with which a conjugate radical combines is always dependent upon the number of atoms of the positive element present in the substance. Both together—for instance, oxygen and methyl, in the oxide of cacodyl, and in cacodylic acid—form a whole, in such a way, that their sum is equal to the number of oxygen atoms in those oxides of arsenic which are the analogues of oxide of cacodyl and of cacodylic acid, viz., arsenious acid, and arsenic acid. It is therefore obvious that the number of positive and negative atoms together, present in the compounds of a conjugate radical, is never greater than the highest number of negative atoms—oxygen, chlorine, &c.,—that the respective positive element is capable of combining with in its normal state. The oxide of tri-ethyl-antimony $(C^4\,H^5)_3\,Sb\,O^2$, and the oxide of tetra-ethyl-antimony, correspond with the acids of antimony. The oxido of ethyl-tin $(C^4\,H^5)\,Sn\,O$, corresponds with the oxide of tin; methyl-zinc $(C^2\,H^3)\,Zn$, with oxide of zinc. It is especially interesting, in reference to the latter substance, to observe that, notwithstanding its great affinity for oxygen, it does not, as is the case with ethyl-tin, give rise to a basic oxide, and, for this reason, that the metal zinc itself does not combine with more than one atom of negative elements.

Professor Frankland suggests, as an explanation of this remarkable fact, the assumption that the affinity—or capacity of saturation—of antimony,—for instance, in antimonic acid, in oxide of tri-ethyl-antimony and in oxide of tetra-ethyl-antimony, in oxide of antimony and in tri-ethyl-antimony, that of tin, in oxide of tin and in oxide of ethyl tin, as well as that of other elements in analogous compounds,—requires only a given number of atoms, irrespective of their chemical nature.

It is considered that in the oxides, sulphides, chlorides, &c., of the several metals, some, or even all the oxygen, or other negative atoms, may be replaced by an equal number of atoms of some positive element, such as hydrogen, methyl, and perhaps also by the oxygenous acid radicals; and that, in consequence of this remarkable substitution, conjugate compounds would be produced, which would be the oxides of independent conjugate radicals, such as cacodyl, ethyl-tin, methyl-antimony, &c.; or else, when all the oxygen were replaced, either the radicals themselves, trimethyl, antimony, or hydrides, methylides, &c., methyl-zinc, hydride of copper. In every instance the positive characters of an element are considerably increased when it is combined, in the above manner, with hydrogen, methyl, and similar positive radicals. In like manner the capacity of saturation of the oxides, containing several atoms of oxygen, decreases, in relation to acids, when these oxides are bases, and in relation to bases, when they are acids. Oxide of tin $(Sn\,O^2)$ is a weak base, and saturates two atoms of acid; oxide of ethyl-tin $(C^4\,H^5)\,Sn\,O$, has strong basic characters, and combines with only one atom of acid, forming a neutral salt. The tribasic arsenic acid $(3\,HO,\,As\,O^5)$, by the substitution of two atoms of methyl for two atoms of oxygen, becomes monobasic cacodylic acid $(HO,\,[C^2\,H^3]_2\,As\,O^3)$, or dimethyl arsenic acid. In a similar manner, when an atom of hydrogen is substituted for an atom of oxygen in tribasic phosphoric acid, a bibasic acid—phosphorous or hydrophosphoric acid $(2\,HO,\,HPO^4)$—is produced; and by the further substitution of another atom of hydrogen for an atom of oxygen, a monobasic acid, hypophosphorous or dihydrophosphoric acid ($HO, H^2 PO^3$) is produced. It may be conjectured that the succeeding substitution product ($H^3 PO$) would be an indifferent substance, or at the most of very weak basic character, and that the further substitution product ($H^4 PO$) would be a base.

Since selenium and tellurium, together with the ethylic radicals, give rise to conjugate radicals analogous to those containing metals, phosphorus, or nitrogen, as has been shown by Professor Wöhler, it may be assumed as certain that analogous conjugate radicals containing sulphur may be produced. Professor Kopp is of opinion that the substances known as methyldithionic acid, phenyldithionic acid, naphthyldithionic acid, are oxygen compounds of such conjugate sulphurous radicals, consisting of two atoms of sulphur associated with one atom of an ethylic radical, and that these acids—for instance, methyldithionic acid or monobasic methylsulphuric acid ($HO\ [C^2 H^3]\ S^2 O^6$)—bear the same kind of relation to bibasic sulphuric acid ($2\ HO,\ S^2 O^6$) that monobasic dimethyl arsenic acid ($HO\ [C^2 H^3]_2\ As\ O^3$) bears to tribasic arsenic acid ($3\ HO,\ As\ O^5$). The acid which corresponds to, and contains one atom of methyl in the place of the sulphuric acid, or one of the six atoms of oxygen bibasic, is capable of neutralizing only one atom of base. The bibasic acid obtained by Professor Hofmann, by introducing two atoms of anhydrous sulphuric acid $(2\ SO^3)_3$ to methyl-sulphuric acid ($HO\ [C^2 H^2]\ S^4 O^5$), and represented by the formula $2\ HO,\ C^2 H^2 S^4 O^{10}$, is regarded by Professor Kopp as being a double acid, analogous in constitution to the benzosulphuric and acetosulphuric acids, as viewed by him.

Benzosulphuric Acid.	Acetosulphuric Acid.	New Acid.
$2\ HO \left\{ \begin{matrix} C^{12} \left(\begin{matrix} H^4 \\ SO^2 \end{matrix} \right) C^2 O^3 \\ SO^3 \end{matrix} \right.$	$2\ HO \left\{ \begin{matrix} C^2 \left[\begin{matrix} H^2 \\ SO^2 \end{matrix} \right] C^2 O^3 \\ SO^3 \end{matrix} \right.$	$2\ HO \left\{ \begin{matrix} C^2 \left[\begin{matrix} H^2 \\ SO^2 \end{matrix} \right] C^2 O^3 \\ SO^3 \end{matrix} \right.$

The assumption that methyldithionic acid contains a conjugate sulphurous radical, having the composition $(C^2 H^3) S^2$, is not in any way opposed to the general custom of representing this acid as sulphuric acid, in which one atom of methyl is substituted for an atom of oxygen, and which has thus become monobasic.

The above considerations have led Professors Kopp and Frankland to the opinion that, in a similar manner, hydrogen or ethylic radicals may be substituted for some of the oxygen in carbonic acid ($C^2 O^4$). Supposing one atom of oxygen to be replaced by hydrogen, methyl, ethyl, &c., the substances produced would be the fat acids. In like manner the acids of the series $HO,\ C^n H^{n-3} O^3$, such as acrylic acid, &c.; and the acids analogous to benzoic acid, &c., as well as other monobasic acids of analogous composition, might all be referred to carbonic acid as the type. There are a number of facts referring to the production and chemical characters of the above-named acids which would furnish good evidence for the probability of this view; but the question which it raises is of such importance that nothing short of positive proof, in the production of these acids direct from carbonic acid, would be sufficient ground for its adoption.

The following formulæ will indicate what other classes of substances are regarded by Professor Kopp to bear a relation to carbonic acid similar to that existing between the acids already spoken of. It must be observed that, in these formulæ, the symbols of those positive elements, which occupy the place of the oxygen atoms of the inorganic oxygen compound, are always placed to the left of the symbol representing the radical of the inorganic oxide:—

Dimethyl arsenic acid is represented in accordance with the views above described by the formula

HO, $[C^2 H^3]_2$ As O^3, instead of the formula HO, As $\left\{ \begin{matrix} [C^2 H^3]^2 \\ O^3 \end{matrix} \right\}$, and in conformity with this, acetic acid or methyl carbonic acid is represented by the usual formula HO, $[C^2 H^3] C^2 O^3$.

If, as is supposed, a positive radical may be substituted for a second atom of oxygen in carbonic acid $C^2 O^4$, an indifferent substance would probably be produced. If the elements substituted for the second atom of oxygen were hydrogen, an aldehyde would be obtained.

Conversion of Aldehydes into Alcohols. By Professor LIMPRICHT.—The production of different alcohols, or the discovery of new methods of obtaining those already known, have long received much attention from chemists, and recently several important results have been obtained, such as the production of benzoic alcohol from bitter almond oil and toluol, by M. Cannizzaro; of ethylic alcohol from elayl, by M. Bertholet; of allyl alcohol from iodide of propylen, by MM. Bertholet, Zinin, Cahours, and Professor Hofmann; and of glycol, the first biacid alcohol, by Wurtz. Moreover, the investigations of glycerine and mannite by Bertholet, have led to a knowledge of so many ways in which the alcohol series may be completed, and of so many of the characters peculiar to this series, that there is no doubt this subject will be followed out by many chemists. Professor Limpricht has succeeded in producing the alcohol corresponding to benzoic acid from chlorobenzol, which he regards as $C^{14} H^6 Cl^2$. He finds that it is not a substitution product of bitter almond oil, but the chloride of a biacid alcohol, which he calls benzol alcohol, $C^{14} H^8 O^4$.

The ready convertibility of benzoic alcohol into benzoic acid rendered the examination difficult.

Chlorobenzol is not affected by sodium, even when boiled with it. Concentrated alcoholic solution of potash does not act upon it in the cold, but at 212° F. converts it into chloride of sodium and bitter almond oil. Chlorobenzol may be distilled without alteration in a current of dry ammoniacal gas, but when heated to 212° F., with solution of ammonia, chloride of ammonium and bitter almond oil are produced.

When an alcoholic solution of chlorobenzol is heated to 212° F. for some time, with two equivalents of alcoholate of soda, there are produced chloride of sodium, and a double ether—ethylbenzoic ether—which remains in solution, and is obtained by fractional distillation at 412° F. This substance is a pleasant-smelling oil, the constitution of which may be expressed by the formula :—

$$\left. \begin{matrix} C^{14} H^5 \\ [C^4 H^5]_2 \end{matrix} \right\} O^4$$

When chlorobenzol is brought in contact with silver salts, the compound benzoic ethers are produced. The acetobenzoic ether is crystalline, and has a composition represented by

$$\left. \begin{matrix} C^{14} H^6 \\ [C^4 H^3 O^2]_2 \end{matrix} \right\} O^4$$

and if both atoms of oxygen were replaced by an ethylic radical, the product would be an acetone. In this way there may be derived from bibasic carbonic acid,

$$2 HO, C^2 O^4$$

Monobasic Acids.	Aldehydes.	Acetones.
HO $[C^2 H^3] C^2 O^3$	$\left. \begin{matrix} C^2 H^3 \\ H \end{matrix} \right\} C^2 O^2$	$\left. \begin{matrix} C^2 H^3 \\ C^2 H^3 \end{matrix} \right\} C^2 O^2$
Methyl carbonic acid, or acetic acid.	Methyl hydrocarbon, oxide, or aldehyde.	Dimethyl carbonic oxide, or acetone.

$HO\,[C^{12}\,H^{5}]\,C^{2}\,O^{3}$	$\left.\begin{matrix}C^{12}\,H^{5}\\ H\end{matrix}\right\}C^{2}\,O^{2}$	$\left.\begin{matrix}C^{12}\,H^{5}\\ C^{12}\,H^{5}\end{matrix}\right\}C^{2}\,O^{2}$
Phenyl carbonic acid, or benzoic acid.	Phenyl hydrocarbonic oxide, or hydride of benzoyl.	Diphenyl carbonic oxide, or benzophenon.

Several characters of carbonic acid, especially its relations to carbonic oxide and chlorocarbonic acid, indicate clearly that two atoms of its oxygen—those which belong to the lower oxide, $C^{2}\,O^{2}$—are in a state of more stable combination than the other two. Consistently with this fact, it is found that in those derivatives of carbonic acid, where one of the more easily displaceable atoms of oxygen is replaced by a positive radical, there is one of the three remaining atoms of oxygen, which may be more readily replaced by negative elements than either of the other two. Thus, for instance, in the conversion of acetic acid into sulphacetic acid ($HS\,[C^{2}\,H^{3}]\,C^{2}\,O^{2}\,S$), or into chloride of acetoxyle ($[C^{2}\,H^{3}]\,C^{2}\,O^{2}\,Cl$). It is interesting, in this respect, to perceive how, by the substitution of a positive element for one of the four atoms of oxygen in bibasic carbonic acid, there may be produced monobasic acid oxides of new oxygenous radicals, which latter may be substituted for hydrogen in many compounds, particularly the ammonias, and which may, like other radicals, be transferred unaltered from the exterior third atom of oxygen to other elements. To express this relation symbolically by rational formulæ, the neutral carbonates would be represented by $2\,MO\,[C^{2}\,O^{2}]\,O^{2}$, or perhaps more correctly by

$$2\,MO\left\{\begin{matrix}CO,\,O\\ CO,\,O\end{matrix}\right.$$

and the neutral acetates by

$$MO\,[C^{2}\,H^{3}]\,C^{2}\,O^{2},\,O.$$

The above considerations lead to the question whether it may not be possible to replace three atoms of the oxygen in carbonic acid by positive radicals? If this is the case, the ethers and alcohols might, in the same manner, be regarded as derivatives of carbonic acid.—*Annalen der Chemie und Pharmacie*, ci. 291.

GEOLOGY.

The Pikermi Fossils. By CHAS. MACLAREN, Esq.—In September last I gave an account of a collection of fossils brought to Paris from Greece by Messrs Lartet and Gaudry, and described, in a memoir submitted to the Academy of Sciences. They were found at Pikermi, a village twelve miles E.N.E. from Athens (supposed by Major Leake to occupy the site of the ancient Attic Demus Epacria.) It stands at the south-east foot of Pentelicus, in a ravine cut by streams descending from that mountain, and about four miles from the Ægean Sea. The collection is remarkable for the singular variety of species it includes, of which we mentioned the following:—Semnopithecus, a monkey; a hedgehog, or perhaps castor; two giraffes, one taller than the living species; a huge edent quadruped, resembling the sloth in form, named by them *Macrotherium Pentelicum*, as large as an elephant; with various bones of gallinaceous birds; but no fishes or reptiles. They consider the deposit as intermediate between the Molasse and the Subappenine marls—miocene or lower pliocene.

The fame of these fossils had also spread to Germany, and Dr Roth, a Bavarian, after spending many months in exploring the deposit, has brought home a rich collection of the bones, which are described by himself and M. Wagner in a report to the Royal Academy of Munich. They make a large addition to those above named, including remains referable to no less than nineteen distinct species of quadrupeds. The monkey, in Dr Roth's opinion, is not the Semnopithecus, but an animal intermediate between that and the Gibbon. He found five species of carnivora:—1. A viverrine, which he has named "Ictitherium;" 2. A glutton, (*Gulo primigenius*); 3. A hyena (*H. eximia*); 4. A wolf, smaller than the living European one; 5. A leonine animal (*Machærodus*), larger than the living lion or tiger; 6. A rodent (*Lamprodon*, beaver), believed to be a hedgehog by Lartet; 7. An edent, the Macrotherium previously mentioned; 8. The *Hippotherium gracile*, or ancient horse, whose bones were so abundant as to enable Dr Roth to construct a complete skeleton; 9. Three jaw-bones of an animal termed "Hipparion," a doubtful species, supposed to have been a three-toed horse; 10. *Sus erymanthus*, a hog larger than the wild boar; 11. The femur of a Mastodon; 12. Part of a cranium with five teeth of a rhinoceros. Of ruminant animals there are the remains of five species of antelopes, a goat, and an ox (*Bos Marathonius*) larger than the bison. Of the nineteen species of extinct animals exhumed from this rich deposit, thirteen are considered new. Dr Roth met with no bats or insectivora, and Lartet and Gaudry, as already stated, found no fishes or reptiles. The bones were much broken, and no complete skeleton was found with all the parts united.

Now, this singular assemblage of bones was found at the foot of Pentelicus, on the south side, at a point where several streams unite, just in the position where we would expect to find them if the animals had lived and died on the mountain, and left their spoils to be swept down by rains and torrents, and buried in the mud and sand they brought with them. Is this, then, a picture of the fauna of Attica towards the end of the tertiary period—thousands of years before man existed? Were races so dissimilar denizens of the mountain at the same time? Did the giraffe, the monkey, the horse, the ox, the goat, the antelope, the hog, dwell in company with the mastodon, the rhinoceros, the lion, the wolf, the hyæna?—and upon a single mountain of very limited extent? In Major Leake's map of Attica, Pentelicus is only nine or ten miles long, its breadth cannot exceed three miles, and its height, if our memory may be trusted, does not exceed 3000 feet.* In the living world are there spots where a fauna so diversified and heterogeneous exists within so narrow a space—narrow even if the animals belonged to all Greece? Does it not look rather like a group collected from different countries and different climates? This is, in reality, the conclusion to which Messrs Lartet and Gaudry arrived. Founding on the apparent improbability of so many and such gigantic animals living on a peninsula so narrow as Greece, and so intersected by elevated chains, they have been led to suppose that the Greece of our days and its isles are only the *debris* of a great continent (which they term Greco-Asiatic), now buried under the waves of the Archipelago and the Mediterranean. It was in this state when the hippurite limestone (a member of the chalk formation) was deposited, and afterwards the nummulitic or lower tertiary. The whole was then raised up, forming one continuous range of land with Asia Minor. Upon this land the extinct quadrupeds of Pikermi lived, and here the animals of Armenia, Syria, and Arabia might meet and mingle with those of Illyria and Thessaly. Subsequently, two great lines of fracture, nearly at right angles to each other, shattered this Greco-Asiatic continent, sinking the part of it which forms the Ægean Sea, separating part into islands, and leaving a Greece something like the present. A yet later movement, "de bascule," or see-saw, like the two ends of a scale-beam, depressed the southern part of the country under the sea, about the Subappenine period, and forced the land animals to seek refuge in the mountains; those in or near the plain of Athens escaped to Pentelicus, where, cooped up within a narrow space, they perished from insufficiency of food, and their bones, exposed to the elements, were washed down by rains and torrents to the ravine of Pikermi. The bones, with the exception of a monkey, are those of quadrupeds only, because we presume the slow-paced reptiles had not time to save themselves; and no fish are found, probably because the Subappenine sea had not remained long enough at the foot of the mountain to allow the finny tribes to settle and breed.

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* A table attached to the French map of Greece makes the height of Mount Hymettus 1028 metres, or 3373 feet. That of Pentelicus is not given, but the writer of these notes, who passed the west end of the mountain in 1847 on his way to Marathon, estimated its height as rather less than that of Hymettus.

This hypothesis will strike most persons as very complicated, and somewhat extravagant, if not fantastic. All the assumed movements of the crust of the earth are perhaps separately consistent with geologic science; but we can scarcely conceive that so many changes, and so great, followed each other so closely, at one spot, and within one limited period. And a second question may be raised, whether they are all indispensable to an explanation of the phenomena? We may suppose, for instance, that the variety of animals whose bones are mingled at Pikermi were *not* all contemporary occupants of Attica. The gentle herbivorous races probably came in first, and the carnivorous, which cannot subsist without prey to feed on, followed them, devoured the weaker, and drove out the stronger, or became co-tenants with them of the mountain. But does the variety of genera afford such clear evidence of a wide region, that it was necessary to *imagine* a Greco-Asiatic continent to make room for them, and then to create a temporary flood to drive them all into one locality?

Discussion, however, in our case is rather premature, as we have not seen the memoir of Messrs Gaudry and Lartet, but merely the abstract given in the *Institute* journal. And in justice to M. Lartet, it should be stated that he is not one of the enthusiasts who erect vast speculations on a very small basis of knowledge. On the contrary, few men are so well qualified by their previous labours to throw light on the problem of the Pikermi bones. We owe to him nearly all our knowledge of the still richer and more varied collection of fossils discovered in the lacustrine deposit at Sansan, close to the north foot of the Pyrenees, which is as great a riddle as that of Pikermi, and not yet solved. From that spot, and the adjacent localities of Simorre, Lombez, and Tournan (department of Gers), M. Lartet disinterred no less than ninety-eight genera, subgenera, or species of mammalia and reptiles, a gigantic bird (*Pelagornis*), and some fresh-water fishes. Among the quadrupeds are nearly all those found fossil over the rest of France, and representatives of nearly all the mammalian family—the plantigrade carnivora, except the bears properly so called, the digitigrade carnivora, the Edentata, Rodentia, Pachydermata, and Ruminantia, and also a bat and a monkey. Among the genera belonging to these families we have the rhinoceros, elephant, mastodon, deer, antelope, tapir, hog, dog, wolf, civet, marten, hyæna, ox, palæotherium, anoplotherium, and dinotherium. They exceed the Pikermi fossils in variety as much as the Pyrenées exceed Pentelicus in length and breadth. The late M. Prévost, an able geologist, attempted to explain how so many land animals were swept into one small fresh-water lake (*Bulletin de la Société Géologique*, 1845-6, p. 338), but D'Archiac, a very high authority, pronounces his explanation a failure.—*Scotsman.*

On the Origin of Greensand, and its Formation in the Oceans of the Present Epoch. By Professor J. W. Bailey.—As an introduction to the subject of this paper, it is proper to refer to various observations which have been made of facts intimately related to those which I wish to present. That the calcareous shells of the Polythalamia are sometimes replaced by silica, appears to have been first noticed by Ehrenberg, who says:—

"I may here remark, that my continued researches on the Polythalamia of the chalk have convinced me that very frequently in the earthy coating of flints, which is partly calcareous and partly siliceous, the original calcareous shelled animal forms have exchanged their lime for silex without undergoing any alteration in figure, so that while some are readily dissolved by an acid, others remain insoluble; but in chalk itself all similar forms are immediately dissolved."

The first notice of *casts* of the cells and the soft parts of the Polythalamia was published by myself in the *American Journal of Science* for 1845, where I stated as follows:—

"The specimens from Fort Washington presented me with what I believe have never been before noticed, viz., distinct *casts* of Polythalamia. That these minute and perishable shells should, when destroyed by chemical changes, ever leave behind them indestructible memorials of their existence, was scarcely to be expected, yet these casts of Polythalamia are abundant and easily to be recognised in some of the Eocene marls from Fort Washington."

Dr Mantell also noticed the occurrence of casts of Polythalamia and their soft parts, preserved in flint and chalk, and communicated an account of them to the Royal Society of London. To Ehrenberg, however, appears to be due the credit of first distinctly announcing the connection between the Polythalamia and the formation of greensand, thus throwing the first light upon the origin of a substance which has long been a puzzle to geologists. In a notice given by this distinguished observer upon the nature of the matrix of the bones of the Zeuglodon from Alabama, he says:—

"That Greensand, in all the numerous relations in which I have as yet examined it, has been recognised as due to the filling up of organic cells, as a formation of stony casts mostly of Polythalamia, was stated in July of the preceding year." He then refers to the nummulite limestone of Traunstein, in Bavaria, as rich in green opal-like casts of well-preserved Polythalamian forms, and mentions them as also occurring, but more rarely, in the glauconite limestones of France. He then proceeds to give an account of his detection of similar casts in the limestone adhering to the bones of the Zeuglodon from Alabama, and states that this limestone abounds in well-preserved brown, green, and whitish stony casts of recognisable Polythalamia. This limestone is yellowish, and under a lens appears spotted with green. These green spots are the greensand casts of Polythalamia, and they often form as much as one-third of the mass. By solution in dilute chlorohydric acid, the greensand grains are left, mixed with quartzose sand, and with a light yellowish mud. The latter is easily removed by washing and decantation. The casts thus obtained are so perfect that not only the genus, but often the species of the Polythalamia, can be recognised. Mingled with these are frequently found spiral, or corkscrew-like bodies, which Ehrenberg considers as casts of the shells of young Mollusks.

With reference to the perfection of these casts of the Polythalamia, and the light they throw upon the structure of these minute animals, Ehrenberg remarks:—

"The formation of the greensand consists in a gradual filling up of the interior space of the minute bodies with a green-coloured, opal-like mass, which forms therein as a cast. It is a peculiar species of natural injection, and is often so perfect, that not only the large and coarse cells, but also the very finest canals of the cell walls, and all their connecting tubes, are thus petrified, and separately exhibited. By no artificial method can such fine and perfect injections be obtained."

Having repeated the experiments of Ehrenberg upon the Zeuglodon limestone, I can confirm his statements in every particular, and would only add, that besides the casts of Polythalamia and small spiral Mollusks, there is also a considerable number of green, red, and whitish casts of minute anastomosing Tubuli, resembling casts of the holes made by burrowing sponges (*Cliona*) and worms. In the Berlin *Monats-Bericht*, for July 1855, Ehrenberg gives an account of very perfect casts of Nummulites, from Bavaria and from France, showing not only chambers connected by a spiral siphuncle, but also a complicated system of branching vessels. He also gave at the same time an account of a method he had applied for the purpose of colouring certain glass-like casts of Polythalamia, which he had found in white tertiary limestone from Java. This method consists in heating them in a solution of nitrate of iron, by means of which they can be made to assume different shades of yellow and brownish red, still retaining sufficient transparency when mounted in balsam to show the connection of the different parts. The interesting observations of Ehrenberg, which are alluded to above, have led me to examine a number of the cretaceous and tertiary rocks of North America in search of greensand and other casts of Polythalamia, &c. The following results were obtained:—

1*st*, The yellowish limestone of the cretaceous deposits of New Jersey occurring with *Teredo tibialis*, &c., at Mullica Hill, and near Mount Holly, is very rich in greensand casts of Polythalamia and of the tubuliform bodies above alluded to. 2*d*, Cretaceous rocks from Western Texas yielded a considerable number of fine greensand and other casts of Polythalamia and Tubuli. 3*d*, Limestone from Selma, Alabama, gave similar results. 4*th*, Eocene limestone, from near Charleston, S. C., gave abundance of similar casts. 5*th*, A few good greensand casts of Polythalamia were found in the residue left on dissolving a specimen of marl from the Artesian Well at Charleston, S. C.; depth 140 feet. 6*th*, Abundance of organic casts, in greensand, &c., of Polythalamia, Tubuli, and of the *cavities of corals*, were found in the specimen of yellowish limestone, adhering to a specimen of *Scutella Lyellii* from the Eocene of North Carolina. 7*th*, Similar casts of Polythalamia, Tubuli, and of the *cavities of Corals*, and species of Encrinitis, were found abundantly in a whitish limestone adhering to a specimen of *Ostrea sellæformis* from the Eocene of South Carolina. The last two specimens scarcely gave any indications of the presence of greensand before they were treated with dilute acid, but left an abundant deposit of it when the calcareous portions were dissolved out. All the above-mentioned specimens contained well-preserved and perfect shells of Polythalamia. It appears from the above, that the occurrence of well-defined organic casts, composed of greensand, is by no means rare in the fossil state.

I come now to the main object of this paper, which is to announce that the formation of precisely similar greensand and other casts of Polythalamia, Mollusks, and Tubuli, is now going on in the deposits of the present ocean. In an interesting report by Count Pourtales, upon some specimens of soundings obtained by the U. S. Coast Survey in the exploration of the Gulf Stream, the sounding from Lat. 31° 32', Long. 79° 35', depth 150 fathoms, is mentioned as "a mixture in about equal proportions of globigerina and blacksand, probably greensand, as it makes a green mark when crushed on paper." Having examined the specimen alluded to by M. Pourtales, besides many others from the Gulf Stream and Gulf of Mexico, I have found that not only is greensand present at the above locality, but at many others, both in the Gulf Stream and Gulf of Mexico, and that this greensand is often in the form of well-defined casts of Polythalamia, minute Mollusks, and branching Tubuli; and that the same variety of petrifying material is found as in the fossil casts, some being well-defined greensand, others reddish, brownish, or almost white. In some cases I have noticed a single cell, of a spiral Polythalamian cast, to be composed of greensand, while all the others were red or white, or *vice versâ*.

The species of Polythalamia whose casts are thus preserved, are easily recognisable as identical with those whose perfectly preserved shells form the chief part of the soundings. That these are of recent species is proved by the facts that some of them still retain their brilliant red colouring, and that they leave distinct remains of their soft parts when treated with dilute acids. It is not to be supposed, therefore, that these casts are of extinct species washed out of ancient submarine deposits. They are now forming in the muds as they are deposited, and we have thus now going on in the present seas a formation of greensand by processes precisely analogous to those which produced deposits of the same material as long ago as the Silurian epoch. In this connection it is important to observe that Ehrenberg's observations, and my own, establish the fact that *other* organic bodies than Polythalamia produce casts of greensand, and it should also be stated that many of the grains of greensand accompanying the well-defined casts are of wholly unrecognizable forms, having merely a rounded, cracked, lobed, or even coprolitic appearance. Certainly many of these masses, which often compose whole strata, were not formed either in the cavities of Polythalamia or Mollusks. The fact, however, being established beyond a doubt, that greensand does form casts in the cavities of various organic bodies, there is a great probability that all the masses of this substance, however irregular, were formed in connection with organic bodies, and that the chemical changes accompanying the decay of the organic matter have been essentially connected with the deposits in the cavities, of green and red silicates of iron, and of nearly pure silica. It is a curious fact in this connection, that the *siliceous* organisms, such as the Diatomaceæ, Polycistineæ, and Spongiolites, which accompany the Polythalamia in the Gulf Stream, do not appear to have any influence in the formation of casts.

The discovery of Professor Ehrenberg of the connection between organic bodies and the formation of greensand, is one of very great interest; and is one of the many instances which he has given to prove the extensive agency of the minutest beings in producing geological changes.

Artesian Wells on the Plains.—With a view of facilitating the overland intercourse with California, the American War Department, two years ago, despatched Captain Pope, of the Engineers, with a party, to endeavour to procure water by means of Artesian wells on the great plain of Llano Estacado, in the thirty-second parallel of latitude, between New Mexico and the Mesilla Valley.

Captain Pope went out to the scene of his labours in the spring of 1855, from Indianola, by the way of San Antonio, and formed his camp on the banks of the Pecos River, where it is intersected by the thirty-second parallel of latitude. From this point he proceeded with his working parties due east a distance of fifteen miles, and there sunk the first well. From the Pecos River the country seems to the eye to be a perfect level, but instrumental observation shows that there is a rise of about six hundred feet in a distance of thirty-five miles; and from that point, which may be termed the summit of the plain, it continues with a gradual descent eastwardly, to the hills from which run the head waters of several of the forks of the Colorado River.

In sinking the wells Captain Pope found no difficulties in the geological formation. This is entirely composed of alternate strata of indurated clay and cretaceous marls, of every variety of colour, easily bored through, but sufficiently hard to prevent the walls of the boring from falling and incommoding the labour.

The first stream of water was reached at a depth of three hundred and sixty feet, and it rose to a height of seventy feet in the tubing. Continuing the labour through the same formation, the second stream of water was struck at the depth of six hundred and forty-one feet, which rose four hundred feet in the well, or about fifty feet higher than the first stream. These labours demonstrated the existence of water streams beneath the surface; but winter approaching, and the material which he had brought having been exhausted, Captain Pope went into winter quarters on the banks of the Rio Grande.

Having received fresh supplies in the spring of last year, he returned to the Llano, and in April last resumed his labours there. His former results having demonstrated the existence of abundant water beneath the surface, he went five miles eastward from the first well, and there sank the second. In the prosecution of this work he struck the same streams that he had found in sinking the first well, and on reaching a depth of eight hundred and sixty feet he encountered another which rose seven hundred and fifty feet in the tubing. At this point the material was again exhausted, and the small appropriation made by Congress for the experiment had been expended. Captain Pope was therefore obliged to suspend his labours, and await further orders from the government.

The results of this work have been eminently successful, for they demonstrate the feasibility of the plan of procuring water on this great plain by the sinking of Artesian wells, and it is much to be hoped that Congress will make another appropriation to continue and perfect the work. Through the absence of water the Llano Estacado forms a complete barrier to travel between the western towns of Louisiana and Arkansas to New Mexico and the Mesilla Valley, along the line of the thirty-third parallel, by a route which is some hundred of miles shorter than any other. It is covered throughout with gama grass, which is one of the most nutritious of the grasses for cattle, and which has the greater advantage, that it is not killed by the cold of winter, affording abundance of pasture all the year round.—*Well's Annual of Scientific Discovery for* 1857.

Conducting Power of Rocks—Altitude of Mountains not Invariable. By Charles Maclaren.—Mr Hopkins of Cambridge has made some rather interesting experiments on the *conductivity* or conducting power of different substances for heat, of which an account was laid before the Royal Society of London in June last. Without attempting to describe his processes, we give his more important results, and in decimals, the conductivity of "igneous rock" (trap or granite, we presume), saturated with moisture, being taken as unity.

Chalk, in the state of *dry powder*,	·056
Clay, do. do.,	·07
Sand, do. do.,	·15
Sand and clay, do.,	·11

The conductivity of the following rocks is given in two states—*dry*, and *saturated* with water:—

	Dry.	Saturated.
Chalk, in block,	·17	·30
Oolite rock,	·30	·40
Hard compact limestones,	·50	·55
Siliceous New Red sandstone,	·25	·60
Freestone,	·33	·45
Hard compact sandstones (Millstone Grit),	·51	·76
Hard compact old sedimentary,	·50	·61
Igneous rocks,	·53	1·00

The effect of *pressure* on the conducting power of substances was also tried, and proved to be almost nothing. A pressure of 7500 lb. on a square inch of beeswax, spermaceti, and chalk, had no appreciable effect. Uncompressed clay, which had a conducting power of ·26, had the same raised to ·33 by a pressure of 7500 lb.

Sandstone, with conducting power of ·5, divided into strata each 1 foot thick, when compared with a similar mass in one block, had its conducting power diminished 1-20th. When the strata were only 6 inches thick the diminution was 1-10th. The effect of discontinuity of substance is therefore small. Saturation with moisture, on the other hand, produces generally a great effect, as will be seen on comparing the dry and saturated blocks of chalk, the dry and saturated New Red sandstone, and again the dry and saturated "igneous rocks."

These facts have a certain bearing on a geological question—namely, the transmission of heat from the interior of the earth to the crust. The oolite, for instance, conducts heat much better than the chalk, the sandstone better than the oolite, the igneous rock better than the sandstone, and in all cases the rock charged with moisture better than the dry rock. But Mr Hopkins would have added to the value of his paper if he had ascertained by experiment the quantity of water absorbed by each rock at given temperatures, and whether the conductivity is exactly in proportion to the absorption.

In illustration of the use that may be made of the tables, we would refer to certain remarks made by Dr Robinson on a paper read by Professor Hennessy at the recent meeting of the British Association. The subject was "The Direction of Gravity at the Earth's Surface." In alluding to certain supposed local and temporary changes of level, he mentioned the following curious fact:—"He found *the entire mass of rock and hill on which the Armagh Observatory is erected, to be slightly, but to an astronomer quite perceptibly, tilted or canted at one season to the east, at another to the west.* This he at first attributed to the varying power of the sun's radiation to heat and expand the rock throughout the year; but he subsequently had reason to attribute it rather to the infiltration of water to the parts where the clay-slate and limestone rocks met. The varying quantity of this (water) through the year he now believed exerted a powerful hydrostatic energy, by which the position of the rock is slightly varied." With the light furnished by Mr Hopkins' experiments, we may pronounce the explanation satisfactory. Armagh and its observatory stand on a hill at the junction of the mountain limestone with the clay-slate, having, as it were, one leg on the former, and the other on the latter, and both rocks probably reach downwards one or two thousand feet. When rain falls, the one will absorb more water than the other; both will gain an increase of conductive power, but the one which has absorbed most water will have the greatest increase; and being thus the better conductor, will *draw a greater portion of heat from the hot nucleus below to the surface*—will become, in fact, temporarily hotter, and, as a consequence, *expand more than the other.* In a word, *both rocks will expand at the wet season; but the best conductor, or most absorbent rock, will expand most, and seem to tilt the hill to one side; at the dry season it will subside most, and the hill will seem to be tilted in the opposite direction.*

The fact is curious, and not less so are the results deducible from it. *First*, hills are higher at one season than another, a fact we might have supposed, but never could have ascertained by measurement. *Secondly*, they are highest, not as we would have supposed at the hottest season, but at the wettest. *Thirdly*, it is from the *different rates* of expansion of different rocks that this has been discovered; had the limestone and clay-slate expanded equably, or had Armagh Observatory stood on a hill of homogeneous rock, it would have remained unknown. *Fourthly*, though the phenomenon is in the strictest sense *terrestrial*, it is by converse with the *heavens* that it has been made known to us. A variation of probably a second, or less, in the right ascension of three or four stars, observed at different seasons, no doubt revealed the fact to the sagacious astronomer of Armagh, and even enabled him to divine its cause; which has been confirmed as the true cause, and placed in a clearer light by the experiments of Mr Hopkins. One useful lesson may be learned from the discovery—to be careful to erect Observatories on a homogeneous foundation.

METEOROLOGY.

To the Editor of the Edinburgh Philosophical Journal.

EDINBURGH, 25th Nov. 1857.

DEAR SIR.—I have this moment received from Mr Forbes of Culloden the accompanying very interesting table and notes regarding the phenomena attending the very high barometer, which all must have remarked, during the second week of November. It is to Mr Birt chiefly that the scientific world is indebted for the discovery of a great atmospheric wave which passes over these islands about the middle of November, and to which he has given the title of "the Great November Wave." He has shown that this wave probably passes over the whole of Europe, that it extends in a direction from N.E. to S.W., that the direction of its progress is from N.W. to S.E., at right angles to the line of the wave, and that it moves with a velocity of about 19 miles an hour. From the observations made, this wave is of enormous size, and as it takes about 14 days to pass over one spot, its total breadth cannot be less than six thousand miles. The observations of Mr Forbes refer to the crest of the wave; but he has rendered these doubly valuable by appending the simultaneous readings of the wet and dry bulb thermometers, the direction and force of the wind, the form and amount of cloud, and the direction of the upper currents as indicated by the movement of the upper strata of clouds. By these it appears that during the period at which the barometric pressure was highest, south-west winds prevailed both on the surface and in the upper strata of the air, and that the air was unusually loaded with moisture. Thus the dry-bulb thermometer, during the period of observation, had a mean temperature of 44°·2, that of the wet bulb being 43°·2, showing a mean difference of only one degree. The dew point temperature was consequently 42°, the elastic force of vapour ·267 of an inch, the weight of vapour in a cubic foot of air 3·04 grs., so that it required only 0·28 of a grain of aqueous vapour fully to saturate with moisture a cubic foot of air; and consequently the mean degree of humidity of the atmosphere was so high as 92°.

Now all these states are just the very opposite of what usually prevails when such a high barometric pressure is dependent on causes which are on or near the earth's surface. With a S.W. wind, the barometer (or atmospheric pressure) is almost always low, and the same occurs when the air is so loaded with moisture as these observations show it to have been. Sir John Herschel therefore considers these great atmospheric waves to be rather dependent on great internal displacements of the atmosphere; "the result of winds diverted from their course, or to great local disturbances of temperature due to a concurrence of circumstances which may be termed casual, forasmuch as we cannot trace their laws."

This great wave, as it has a notable crest, so it has a corresponding trough, and in a short note which accompanied these observations, dated 23d November, Mr Forbes says, "A heavy storm to-day from the N.E., with barometer (corrected and reduced) down to 28·989 inches at 1 P.M., showing a range from its greatest height on the 11th, of 1·768 inches." Very nearly the same range was noticed in Edinburgh.

Trusting these few remarks will prove interesting to your readers, and induce them to take a greater interest in Mr Forbes' valuable table. I remain, ever truly yours, JAMES STARK.

Great height of the Barometer.

On Wednesday the 11th inst., the barometer attained an elevation rather unusual for this month of the year. The following table, compiled from the Meteorological Register kept at Culloden, shows the fluctuations of the mercurial column for each hour on the 11th, and for every third hour on the following day. The readings being reduced to the temperature of 32° Fahr., and to the level of the sea, can be easily compared with any simultaneous observations taken elsewhere. Lat. 57° 31′ N.; Long. 4° 5′ W.:—

Day and Hour, Local Time.	Barometer corrected to 32° Fah. and reduced to the level of the Sea.	Thermo.		Difference between Therm.	Wind.		Clouds.		
		Dry Bulb.	Wet Bulb.		Direction	Force 0 – 6	Form.	Amt. 0 – 10	Upper Current From.
Nov. 11.									
5 A.M.	30·732	42·2	41·2	1·	S.S.W.	0·1	Ci.-st.	0·5	Stationy.
6	·732	41·6	40·7	0·9	S.S.W.	0·1	Ci.-st.	0·7	Stationy.
7	– ·731	41·0	40·2	0·8	S.S.W.	0·1	Ci.-st.	1·	Stationy.
8	·746	41·3	40·9	0·4	Calm.	0·	Scud.	2·	S.W.
9	·746	41·9	41·6	0·3	S.S.W.	0·1	Ci.-st.	3·	S.W.
10	+ ·748	43·0	42·7	0·3	Calm.	0·	Ci.-st. st.	5·	S.W.
11	– ·747	44·1	43·4	0·7	Calm.	0·	Ci.-st. st.	7·7	S.W.
Noon.	+ ·752	46·0	43·9	2·1	Calm.	0·	Ci.-st.	9·	S.W.
1 P.M.	·746	46·2	45·4	0·8	Calm.	0·	Ci.-st. st.	9·	S.W.
2	·746	46·3	45·	1·3	Calm.	0·	Ci.-st. st.	8·	S.W.
3	·744	46·0	44·2	1·8	Calm.	0·	Ci.-st. st.	6·	W.S.W.
4	·744	44·2	42·9	1·3	Calm.	0·	Ci.-st. st.	6·5	W.S.W.
5	– ·743	44·2	42·9	1·3	S.W.	0·2	Ci.-st. st.	9·5	W.S.W.
6	·744	44·6	43·1	1·5	S.W.	0·2	Stratus.	9·3	W.S.W.
7	·749	44·5	43·1	1·4	S.W.	0·1	Stratus.	8·5	W.S.W.
8	·753	44.5	43·1	1·4	S.W.	0·4	Stratus.	9·	W.S.W.
9	+ ·757	44·8	43·6	1·2	S.W.	0·1	Stratus.	9·	W.S.W.
10	– ·747	44·1	42·9	1·2	S.W.	0·5	Scud.	4·5	W.S.W.
11	+ ·750	43·4	42·5	0·9	S.W.	0·1	Stratus.	8·5	W.S.W.
Midnight	·745	44·1	42·9	1·2	S.W.	0·7	Stratus.	9·7	W.S.W
Nov. 12.									
3 A.M.	·734	43·1	42·1	1·	S.W.	0·2	Ci.-st.	5·5	Stationy.
6	– ·722	43·3	42·8	0·5	S.S.W.	1·	Ci. Ci.-st.	1·	Stationy.
9	+ ·726	43·9	43·	0·9	S.W.	0.3	Ci. Ci.-st.	3·	N.N.W.
Noon.	·681	48·0	46·6	1·4	S.W.	0·7	Ci. Ci.-st.	4·	Stationy.
3 P.M.	– ·638	47·8	46·2	1·6	S.W.	1·	Ci.-st.	5·	W.S.W.
6	+ ·640	45·1	44·1	1·	S.W.	0·6	Stratus.	3·	W.S.W.
9	·601	44·4	43·6	0·8	S.W.	1·2	Stratus.	6·5	W.S.W.
Midnight	·556	45·6	44·5	1·1	S.W.	1·5	Stratus.	10·	W.S.W.

General Remarks.

November 11.—Night of the 10th nearly clear, fine; faint aurora over northern horizon; fair; day very fine and settled; generally calm, but occasionally a very gentle breeze from S.S.W. to S.W.; morning nearly clear, and partly so about 3 P.M., rest of the day nearly cloudy; a little sunshine in morning and evening; fair.

November 12.—Night of the 11th fine; partially cloudy; a gentle breeze; fair; day very fine; dry and pleasant; sky partially covered with cirrus and cirro-stratus clouds, in which appeared a broken solar halo at 10 and 11·30 A.M.; more clouded by evening, and wind rising; fair.

On the 4th of March 1854 the barometer attained the extraordinary height of 30·878 inches, but its elevation on the 11th inst. is the greatest recorded in any November during the last seventeen years, the nearest approach to it being on the 12th of the same month in 1848; when the mercurial column, corrected to 32° Fahr., and reduced to the level of the sea, stood at 30·685 inches.

NOTE.—The barometer used in taking these observations is a standard used by the British Association; tube in brass, and ·31 of an inch in diameter; cistern adjusted by a fine point, which dips into the mercury. The readings are severally corrected for capillarity, and reduced to 32° Fahr. by means of the tables in the Royal Society's Report on Physics and Meteorology, published in 1840.

Table showing the fluctuations of the Barometer, and the direction and force of the Wind, during the storm of Monday, November 23, 1857.

Lat. 57° 31' N. Culloden. Long. 45° 5' W.

Day and Hour, Local Time.	Barometer corrected to 32° Fah. and reduced to the level of the Sea.	Wind.	
		Direction	Force in lbs. on Square Foot.
Nov. 23.	inches.		
5 A.M.	29·380	Calm.	0·
9	·156	N.E.	0·30
10	·101	N.E.	2·25
11	·052	N.E.	2·25
Noon.	28·989	N.E.	25·
1 P.M.	·990	N.E.	16·
2	29·032	N.E.	25·
3	·070	N.E	9·
4	·135	N.E.	16·
5	·198	N.E.	16·
6	·223	N.E.	9·
7	·267	N.E.	2·25
8	·306	N.E.	2·25
9	·345	N.E.	1·
10	·359	N.E.	2·25

Remarks.

Night of the 22d overcast and rainy; calm; barometer fell ·457 of an inch during the night; morning very rainy, but calm till 9 o'clock, after which hour the wind gradually rose from the N.E., becoming strong by 10, and then blew a gale, with heavy rain, mixed at times with hail, throughout the rest of the day. Quantity of rain registered in the gauge by 4 P.M., fallen since last night, 1·607 inches; faint aurora borealis at 7 and 10 P.M. At the commencement of the storm, the temperature of the air was 44° 3′, but at the time it abated only 35° 2′, having fallen in the interval 9° 1′.

During this storm much ozone seemed to be present in the atmosphere; but for some weeks previous to this date the air was unusually calm, and very little ozone could be detected by the test-papers.

MISCELLANEOUS.

Cetonia aurata and Hydrophobia.—In 1851 M. Guerin Meneville inserted a notice in his *Revue et Magazin de Zoologie*, that in Russia the *Cetonia aurata*, or common Rose Beetle, was used successfully as a remedy against hydrophobia. The beetles were reduced to powder, like cantharides, and administered internally, in doses of greater or less amount, according to the state of the disease and the age and strength of the patient. From time to time since 1851 M. Meneville has introduced other notices mentioning cures of the disease by the above remedy, giving at the same time the anthority for them; and in a late number of the *Revue* for the present year (1857), additional facts are stated, and a request is made to the Academie des Sciences, that it should order an examination to be made of the substance or principle contained in these beetles, which he judged to be analogous to *cantharidine*, for which he proposed the name of *cetonine*. In various parts of the Continent, and in Russia particularly, *rabies* is annually almost a scourge, and in the latter country sportsmen are in the custom of administering a *cetonia* to their dogs. Whether it produces a cure, or even acts only as a preventive, we have no authority for stating; the fact of the administration of the insects only points out the prevailing opinion, and it would certainly be interesting to ascertain if any peculiar active principle existed in any of the Cetonidæ.

Jay from Algeria.—M. Jules Verreaux has figured a new and remarkable jay from Algeria. It is remarkable as being almost the prototype, except in size, of the common jay of Europe, *Garrulus glondarius*. The differences will be best seen by comparing the size of the principal parts. The measurements are French, as given by Verreaux:—

Garrulus glondarius.	Cent.	mill.	*Garrulus minor*, Verr.	Cent.	mill.
Entire Length.........	35	0	Entire Length.........	27	0
Wing....................	18	0	Wing	14	8
Tail	14	4	Tail	13	2
Tarsus....................	4	5	Tarsus....................	3	6

Planetoids.—M. Luther of Bilk discovered another of these small planets circulating between Mars and Jupiter, on the 19th October, raising the number known to fifty. Bilk is the name of an Observatory in Rhenish Prussia, near Dusseldorf, of which M. Luther is the astronomer. —*C. Maclaren.*

Fossil Mammalian Footmarks.—M. Daubree, a French geologist, laid before the Academy of Sciences lately casts of certain impressions found in sandstones of the *Gres Bigarré* (Trias or New Red Sandstone) in the department of Haute Saone. They are compared to some impressions found in Thuringia—namely, those of the Labyrinthodon, a reptile noticed by Sir C. Lyell (*Manual*, p. 342). "They have some resemblance to the paw of a dog, and seem to afford a new proof that mammifers existed when the last beds of the Trias were deposited." That the footmarks may be those of a quadruped is credible, since the *Microlestes antiquus* belongs to this formation, but the Labyrinthodon was also supposed to be a quadruped till Owen pronounced it a reptile. At all events the fact must remain doubtful till some competent authority pronounce an opinion.—*C. Maclaren.*

Volcanic Eruptions.—An official report, sent to the Dutch Government from one of its settlements in the Spice Islands, describes two destructive eruptions in the island of Sangir, north of Celebes, on the 2d and 17th March 1856. Several villages, and a great part of the crops, were destroyed by the lava, or the fragmentary matter ejected, or by the torrents of water which escaped from the sides of the volcano, and 2806 human beings fell victims. No change was observed on the summit of the mountain, but some portions of its sides on the coast had sunk in the sea and disappeared, and in consequence thereof a precipice 70 metres (230 feet) in height had replaced what was formerly a gentle declivity. A translation of the official report was laid before the Academy of Sciences on the 26th October.—*C. Maclaren.*

Meteoric Stones with Detonations.—M. Seguier presented to the Academy of Sciences, on the 2d November, an aerolite which fell at Ormes, in the department of Yonne (106 miles S.E. of Paris). It was a fragment, gray externally, blackish within, and weighing about 4 ounces (125 grammes). It was found by a mason, who was nearly hit by it while standing at work on a scaffold, which it struck, and afterwards sunk an inch or two (quelques centimetres) into the ground. He added that he heard at the same time a detonation and a noise as of a shower of such fragments, believing, he said, that a "hodful of stones had fallen over his head." A striking phenomenon, witnessed by M. Seguier himself, residing within seven miles of the place, led him to attach importance to the mason's story, which only reached him afterwards. About a quarter before five on the afternoon of the same day, when the atmosphere was clear, calm, and cloudless, a loud detonation was heard, which M. Seguier compared to the sound of a cannon-shot of the largest size. It was followed by seven or eight others of equal intensity, and to this succeeded a great noise, resembling the tumbling of ballast into the hold of a ship, accompanied with furious gusts of wind, while the ground trembled so violently that M. Seguier felt a strong tree vibrate against which he leaned for security. The agitation of the ground also made the glasses of garden-frames shake and slide over one another. In a short time the atmosphere returned to a state of tranquillity, when inquiries arose on all sides as to the cause of the phenomenon, and it was then that M. Seguier heard, from a person worthy of credit, of what had occurred to the mason. From another party he learned that a large fire-ball was seen moving towards Ormes at a low elevation. The facts observed render it probable that there was a shower of aerolites, and M. Seguier intends to search for them by digging. A dozen of years ago a meteoric stone, weighing 35 pounds, fell in an adjoining locality. The Academy referred the fragment to a commission for examination.—*C. Maclaren.*

Geoffroy Saint Hilaire.—A statue of this great naturalist has been recently erected at Etampes, his native place, about forty miles south from Paris. Three distinguished *savants*, Dumeril, Serres, and Milne-Edwards, attended the inauguration, and delivered short addresses, commemorating the talents and labours of Saint Hilaire. In 1793, at the early age of twenty-one, he was appointed Professor of Zoology by the recommendation of Hauy and Daubenton. When his nomination was announced to him by the latter, he replied,—"How am I to teach a science that does not exist?" "True," said Daubenton, "it does not exist; it must be created; let the bold task be yours, and yours the glory of enabling us to say, twenty years hence, that France has created zoology." He devoted himself to the work with enthusiasm, and the product of his labours appeared in a long succession of Memoirs which were afterwards embodied in his voluminous *Histoire Naturelle des Mammiferes.* He laboured zealously to enrich the Museum, or Cabinet of Natural History, and to enlarge the menagerie. He was one of the *savants* selected to go with Bonaparte to Egypt, where he employed himself with great diligence in collecting specimens of the higher animal tribes from the delta of the Nile to the cataracts, and along the shores of the Red Sea. When Egypt was conquered by the British his collections and those of the other *savants*, were claimed by a commissary as part of the victors' spoil. "*We will burn them sooner than suffer them to be taken from us* (said Geoffroy), *and write on your forehead the brand of Omar, whose name glares on posterity through the flames of the Alexandrian Library.*" The claim thus roughly repelled was not persisted in, and the treasures gathered in Egypt formed the base of great scientific collections now seen in Paris. Geoffroy continued his labours on his return to France, and fully realized the anticipations of Daubenton; but we must give the result in the words of M. Serres, so characteristic of the taste of our vivacious neighbours. "Geoffroy l'entreprit (the task of creating the science) et les vingt années, n'étaient pas écoulées, que l'Europe savante *inscrivait la Zoologie au rang des titres glorieux de notre nation, dèja si plein de gloire.*—(Charles Maclaren in *Scotsman*).

New Scientific Expedition.—Austria, generally so apathetic in matters of science, has equipped a frigate, the "Novara," for an expedition round the world. It has a complete staff of astronomers, botanists, zoologists, geologists, ethnologists, &c. The crew, with the officers and men of science, includes 357 persons. She is to sail from Trieste, and hold her course by Rio Janeiro, La Plata, the Cape, Bombay, China, Manilla, the Pacific Isles, Panama, and round Cape Horn. The duration of the voyage is expected to be two years. C. M.

Coal in the Rocky Mountains. Letter from W. P. BLAKE, Esq., *of the U.S., to the American Editor.*—"My tour through Texas and New Mexico the past summer proved most interesting and instructive. I spent a few weeks in Santa Fé and the vicinity, observing the geology, and paying special attention to the gold region of the Placer Mountains and to the carboniferous rocks. One of my most interesting results is the determination *by fossils* of the existence of the veritable coal-measures on the *west* slope of the first range of the great Rocky Mountain chain. They contain beds of bituminous coal; and about 25 miles south of Santa Fé *anthracite* is found in a bed thick enough to be profitably worked. Hitherto, you are aware, there has been much doubt about the age of the coal-beds found in these mountains, and beyond. They have been regarded as more recent than the carboniferous. My observations settle the fact that the true coal occurs there. The fossils are identical, specifically, with those in the coal-measures of Missouri. The coal-fields are thus shown to extend 1090 miles west of the Mississippi, and to crop out at an altitude of from 6000 to 7000 feet above the sea,—the limestones being much higher, up to 12,000 feet, as before observed by Marcou.

These coal-seams are accompanied by thin layers of gypsum in dark shales, which, in some places, bear the impress of ferns. The strata are coarse grits and limestones, the latter in thin beds, and usually highly charged with *Producti, Spiriferes, Althyris*, and stems of crinoids and corals.

Dust-Shower at Baghdad.—From a letter of the Hon. C. A. Murray to Sir Charles Lyell.

Baghdad, *May* 23, 1857.

My dear Sir Charles,—We have lately witnessed here a phenomenon so strange, that a brief description of it may not be uninteresting to you. On the 20th instant, a few minutes before 6 p.m. (which is here about an hour before sunset), I was sitting with my Mirza reading some Persian letters, when on a sudden I became sensible of an unusual obscuration of the light on the paper. I jumped up, and going to the window, saw a huge black cloud approaching from the north-west, exactly as if a pall were being drawn over the face of the heavens. It must have travelled with considerable rapidity, for in less than three minutes we were enveloped in total darkness, a darkness more intense than an ordinary midnight, when neither stars nor moon are visible. Groping my way amidst chairs and tables, I succeeded in striking a light, and then feeling assured that a simoom of some kind was coming on, I called to my servants to come up and shut the windows, which were all open, the weather having been previously very sultry. While they were doing so, the wind increased, and bore with it such a dense volume of dust or sand, that before they could succeed in closing the windows the room was entirely filled, so that the tables and furniture were speedily covered. . . . After a short time the black darkness was succeeded by a red lurid gloom, such as I never saw in any part of the world, and which I can only liken in imagination to the effect that might be produced if all London were in conflagration in a heavy November fog; to me it was more striking (I may almost say fearful), than the previous utter darkness, and reminded me of that "darkness visible" in which the poetic genius of Milton placed the demons and horrid shapes of the infernal regions. This lurid fog was doubtless occasioned by the rays of the western sun shining obliquely on the dense mass of red sand or dust which had been raised from some distant desert, and was borne along upon the blast. I inclose you a specimen of the dust. The Arabs here think that it came from the Nejd. The storm seems to have travelled in a circular direction, having appeared first from the south, then south-west, then west, then north-west. After about two hours it had so far passed away, that we were able to open the windows again and breathe the outer air. It cannot have been a simoom, for during those which I have experienced in Arabia and Egypt, the wind is hot and stifling. On the 20th the wind was high; but only oppressive from the dense mass of dust that it carried with it.—I remain, &c.,

Chas. A. Murray.

⁂ Professor J. Quekett, of the Royal College of Surgeons, who examined the specimen of red dust from Baghdad, which accompanied Mr Murray's letter, could detect, under the microscope, only inorganic particles, such as quartz-sand, in the dust. There are no relics of Diatomaceæ apparent; and though a small portion of calcareous matter was present in the sand, yet he could observe no microscopic shells or other organic matter.

OBITUARIES.

Notice of the Life and Writings of Baron Cauchy. By Prof. Kelland.*

In Baron Cauchy the world has lost the last of those eminent cultivators of mathematical science who sprung up in the early part of the present century, formed in the school of Laplace and Lagrange. The names of Poisson, Gauss, Fourier, Abel, Jacobi, and Cauchy, form a constellation of abstract mathematicians such as the world never before saw existing together, and will probably never see again. Augustin-Louis Cauchy was born on the 21st of August 1789, the period of universal confusion throughout France. His father, who was keeper of the archives of the senate, appears to have been exempt from the turmoils which embroiled every grade of society at that time. Perceiving the mathematical bent of his son's mind, he took pains to bring him frequently under the notice of Lagrange. This illustrious philosopher interested himself in the education of the lad, and gave the father a piece of advice which no doubt greatly surprised him, and which, coming from such a source, it is worth our while carefully to note. These were his words:—"Do not allow your son to open a mathematical book, nor to touch a single diagram, until he has finished his classical studies." Sound and excellent advice under the circumstances. Preliminary education has for its object the cultivation of all the faculties, not the development of any one to the exclusion of the others. It fulfils its functions as well when it tends to check and keep down an overwhelming bias in one direction, as when it aims at drawing out the dormant powers in another. The wisdom of the advice of Lagrange may be inferred from the whole life of Cauchy. In his classical studies he was eminently successful, and received the highest award of his class. The taste which he now acquired for languages never forsook him. In his later years he read deeply in patristic theology, and delighted in pouring forth his divinity for the instruction of the young. Nor did his exclusive devotion to classical study stand in the way of his professional advancement. After a single course of mathematics under a public professor, Duret, he presented himself, at the age of sixteen, for the entrance examination of the *Ecole Polytechnique*, and was ranked second on the list.

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* Read before the Royal Society, Dec. 21, 1857.

It is not necessary to trace, step by step, his advance in his profession. Suffice it to say, that he became *ingénieur en chef* in 1823, and was employed on many public works.

Prior to this date, however, he had been brought prominently before the world. The French Institute had proposed as the subject of the Prize Essay for 1816, the determination of the wave motion of a disturbed fluid. M. Poisson, who, as he himself states, had been for a long time engaged on this problem, sent in a first memoir on the subject in October 1815, followed by a second in December. There is reason to suppose that one object which the Institute had in view in proposing this problem was to draw out M. Poisson. That any living man should have succeeded in wresting the prize from him, who was justly regarded as a giant in investigations of the kind, is matter of astonishment to this day. That that man should have been Cauchy, who justly looked up to Poisson as his model for imitation, and who, years after, acknowledges with gratitude his obligations to that great mathematician as the guide of his early career, must have greatly surprised even Poisson himself; yet such was the fact. The prize was awarded to Cauchy on the ground of the greater generality and freedom from limitations which his solution of the problem presented. I am not sure that M. Poisson was satisfied with the decision. At any rate his own memoir was immediately published, whilst that of M. Cauchy, who was not then a member of the Institute, lay twelve years in manuscript. In this case the Institute, by following their ordinary vicious practice, conferred a real benefit on science, by allowing M. Cauchy to add copious notes to his essay. The two works of Poisson and Cauchy now stand together as masterpieces of analytical investigation, and form the starting-points from which all future writers on the subject must commence their progress. Prior to this period M. Cauchy had published several admirable papers on subjects connected with pure geometry; and the proof now afforded of the fertility of his genius would at once have secured him an admission into the Institute, had there been a vacancy. The termination of the brief struggle of the hundred days unhappily too soon created the desired vacancy, in a manner little to the benefit of M. Cauchy, who was named to fill it. The Institute had been remodelled by Napoleon in 1803, and the legitimate monarchy, on their second restoration, at once resolved to re-establish it in its original form. In effecting this re-establishment it is not much to be wondered at that the Government should see fit to strike out the names of two members, Carnot and Monge—names not more distinguished by the brilliant talent of their possessors, than by their connection with that of the first consul Napoleon. Great as was Cauchy's genius, amiable as was his disposition, it could not prevent his sharing in the general feeling of disgust and dissatisfaction at the expulsion of Monge. Connected as the latter had been with the revolution, he had raised his hand when in power only as a shield to protect his colleagues from the proscription of the Reign of Terror. To sit in his place was to participate in the obloquy attached to his removal. Looking at the matter from this distance of time, however, we cannot impute the slightest blame to Cauchy. He was a legitimist by conviction. In the depth of his ardent piety he believed that the interests of religion were bound up with those of the monarchy; and as he never for a moment doubted the propriety of the act which placed his name on the roll, so he accepted the appointment without hesitation, firmly and conscientiously believing that it was his duty so to act.

About the same time he was appointed a professor adjunct in the *Ecole Polytechnique*. He occupied besides two other chairs. The lectures which he delivered are well known to the world under the titles of "*Cours d'Analyse Algebrique*," "*Leçons sur les Calculs, &c.*," "*Resumé des Leçons sur le Calcul Infinitesimal*," "*L'application de l'Analyse à la Théorie des Courbes*." He published also at this period various important memoirs, especially one on integrals taken between imaginary limits.

In 1826, he undertook the Herculean task of conducting and carrying on a scientific periodical, under the title of *Exercises de Mathématiques*, confined exclusively to his own writings. After the lapse of little more than four years the work had advanced into the fifth quarto volume, without any abatement of originality or of interest, when it received a sudden interruption. M. Cauchy, as we have said, was a warm adherent of the legitimate monarchy, and its overthrow was his own. Following the example of its predecessors, the new government demanded an oath of allegiance from all men holding public situations. This oath appears to have made no stringent demands, none which a scientific man might not safely have conceded, whatever his political principles. But M. Cauchy's conscience was tender even to excess; and although he had now a wife and two children depending on him, he resigned all his employments and retired into voluntary exile in Switzerland, sacrificing his prospects "to devotion to the unfortunate and the sincere love of truth." The King of Sardinia, informed of the circumstance, created for him a Chair of Mathematics in Turin. This appointment he accepted, and lectured in the Italian language with great success. There he recommenced the publication of his *Exercises*, under the appellation of *Resumés Analytiques*. Having remained in Turin about two years, the voice of his sovereign (Charles X.) called him to Prague to take part in the education of the Count de Chambord. At Prague he was rejoined by his wife and family; and for the succeeding six years he attached himself to the persons of the royal exiles. Again he resumed his *Exercises;* and having, I believe, plenty of spare time on his hands, he appears to have amused himself with lithography. In this new form he issued his publications; and it is to be feared that a complete set does not exist. I have the impression that M

Cauchy informed me, with his own lips, that he did not himself possess copies of all his lithographed memoirs. At any rate, they are almost unknown even in France.

Charles X. died on the 6th of November 1837; and M. Cauchy's functions as tutor to the Count of Chambord having ceased, he returned to Paris in 1838, and resumed his place at the Institute. He now took the title of Baron Cauchy, but whether by succession or by creation I do not know. Having no public occupation, he divided his time between the pursuits of science and the performance of deeds of benevolence. In both his voluntary labours he was indefatigable. The time he bestowed on each seemed to preclude the possibility of his having a moment for attention to the other. During the last peaceful nineteen years of his life he published in the different volumes of the Institute, and in the *Comptes Rendus*, upwards of FIVE HUNDRED memoirs, besides a multitude of reports and criticisms. This immense mass of work abounds in new thoughts, new methods, and sweeping generalizations, and may be regarded as an immense storehouse from which the next generation of mathematicians will draw their resources. It is to be regretted that M. Cauchy did not concentrate his attention more. Many of his papers are in a very rude state, containing only the germ of an idea, which he failed fully to develope. In fact, during his later years he reminds one a little of Hooke, who was wont to rise at the conclusion of every memoir which he heard, and declare that he had something in store on the same subject. The notation, too, of some of his papers is a notation peculiar to himself; and the methods employed are often those of a new calculus, the *Calcul des Residus*, invented by him, but not generally adopted by mathematicians. All these circumstances will conspire to lock up M. Cauchy's papers for a considerable period. But no one hesitates about their value. In those subjects where the results of his analysis can be easily tested, such as in the determination of the motion of elastic media, with its application to the undulatory theory of light; or in the doctrine of planetary disturbances as applied to the movements of the small planet Pallas, M. Cauchy was, and will continue to be, the received authority.

No sooner had he settled at Sceaux, in the neighbourhood of Paris, than, for the fourth time, he commenced the publication of his *Exercises*, which he continued to the day of his death. The extraordinary amount of work thus performed by one man strikes the mind with astonishment. It is true that many of his papers are but the exhibition in type of the pages of his scribbling book. He had the habit during life of preserving all his loose thoughts and unsuccessful attempts by working constantly on paper bound in volumes. Thus whatever he penned was sure to be preserved. We may perhaps be permitted to regret this circumstance, as its evident tendency was to present a bar to the operation of that polishing process which most writers find so essential to the success of their works. But M. Cauchy was not allowed to remain nineteen years in the silence of the study. On the 13th of November 1839, the *Bureau des Longitudes* called him to the place previously occupied by M. Prony. This was an unfortunate event. It was evident to all those who knew M. Cauchy that he would never consent to take the requisite oaths. Negotiations were accordingly at once set on foot by those who desired his presence amongst them, with the object of inducing the Government to dispense with the formality. Men of science of every shade of political opinion interested themselves in the matter; but without success. The Government did, indeed, consent to reduce the oath to the merest matter of form, but an absolute dispensation it would not concede; and Cauchy was less likely to move towards the opposite party than they towards him. With an obstinacy quite puerile, to use M. Biot's phrase, he doubled on their path at every turn they took to encompass him. His resolve rendered all their efforts hopeless; and finally his appointment was cancelled. Those only who know what Cauchy was capable of will be able to estimate the loss astronomy has sustained from this untoward event.

In 1848 France saw another revolution, and a new republican government. Oaths were now dispensed with, and M. Cauchy resumed his chair of mathematics in the Faculty of Sciences. But the events of the 2d December 1851 once more unseated him. Again the scientific men of France (to their infinite credit be it recorded) used every effort to induce the newly constituted authorities to make his an exceptional case, and dispense with every formality. At first without success; but after a while, when the Emperor had become securely established in his government, he had the good sense to cause M. Cauchy to be restored to his chair, fettered by no conditions. Whether from conscientious scruples or otherwise, it is certain M. Cauchy never appropriated to his own use one farthing of his salary. The whole was devoted to deeds of charity. As the dispenser of blessings to the poor, he knew neither monarchists nor republicans. In the neighbourhood of Sceaux, where he resided, he was the prime mover in every labour of love. On one occasion the mayor remonstrated with him on the prodigality of his beneficence. His reply was. " Be not concerned; I am only the channel; it is the Emperor that pays the money," alluding to his salary as professor.

The scientific character of M. Cauchy requires no exposition. I am content to adopt the judgment of a competent authority, the Dean of Ely, pronounced nearly a quarter of a century ago, which will be fully confirmed by future eulogists. " M. Cauchy, " he says," is justly celebrated for his almost unequalled command over the language of analysis."

With the private life of a scientific man the biographer has properly little to do. But in the present instance, the brilliant virtues of the Christian shine so brightly upon his genius, that the latter, dazzling as it is, fails to eclipse the former. M. Cauchy's labours among the infirm, the destitute, and the young, are the labours of a true apostle. His march always was forward; his watchword always duty. As seen by the eye of the man of science, he was absorbed in study; as seen by the eye of the man of God, he was absorbed in labours of love. In every scheme for the instruction, for the sustentation, for the elevation of his commune, he was ever active, ever devoted. No amount of labour, no sacrifice of time or of money, was too great for him. He was accustomed to wait on the mayor almost daily, and often several times in the day; and he brought with him all his resources of heart, of head, and of purse—now to recommend a poor infirm man to the charity which primarily came from himself; now to suggest the adoption of an orphan whom he had hunted out; now to restore a wounded soldier to his family; now to organize a school; now to forward the working of an hospital. " He had (says the eloquent mayor of Sceaux) two distinct lives—the Christian and the scientific life—each so full, so complete, that it would have served to confer lustre on any name." A characteristic feature in his good works was that truly Christian one, that he conducted them without ostentation and without assuming even the shadow of merit.

A little before his death, aud when it was but too evident that his end was approaching, he was busily engaged with the curé of the parish in arrangements for the benefit of the poeple. Perceiving that he was overtaxing his strength, the curé besought him to take rest, adding, that in so doing, he would second the efforts of those who were praying for his restoration to health. His reply was in these words, and they are the last of his recorded words:—" Dear Sir, men pass away; but their works remain. Pray for the work."

I have a pleasing remembrance of the retired chateau at Sceaux, with its vine-trellised gardens; and of the beaming countenances of M. Cauchy and his agreeable family. In that retreat all was as bright as the summer sky. To the great and good man, whose loss we now lament, it was the dawning brightness of the morn "that shineth more and more unto the perfect day."

Notice of the late Rev. Dr John Fleming, Professor of Natural Science, New College. By ALEXANDER BRYSON, Edinburgh.

This veteran naturalist expired at his house, Sea Grove, near Leith, on Wednesday, 18th November. On the previous day he had lectured with his usual vigour, and talked to his friends with a light-heartedness which to them did not presage so sudden a change. To those who knew him best, the reflection now comes,—Had he known the call was so near, would he have been otherwise, and their own hearts can answer, No. Seldom has it been our experience, or rather happiness, to meet with one such, who talked of death as other than a happy change, and this, not that he did not enjoy the present life—few enjoyed it more. On his return home, between three and four in the afternoon, he was suddenly seized with severe cramp of the extremities, and spasm of the bowels. The pain continued during the night. About 10 A.M. on Wednesday, the pulse became weak and intermitting; the countenance sunk and anxious; extremities cold, with hiccup, abdominal pain, and tympanitis—leading to the suspicion of the rupture of some internal viscus. Two hours afterwards the pain ceased, and he appeared to have fallen asleep, and expired at a quarter before two P.M., less than twenty-three hours from the first seizure. On inspection of the body after death, a simple penetrating ulcer, at the posterior surface of the small curvature of the stomach, near the pylorus, half an inch in diameter, was found, permitting the escape of the contents of the stomach into the abdominal cavity, and causing peritonitis.

Dr Fleming was born at Kirkroad, near Bathgate, in January 1785, where his ancestors were long resident. Few if any of his contemporaries remain from whom may be gleaned the history of his school-boy days. This, at least, is known, he was not distinguished for any position in his class, nor was any love of mere scholastic lore a feature of his after life. But he had an inner life—little sympathized with then—as he rambled about the rocks of Kirkton, and laid up a store of facts which bore rich fruit in his riper years.

He studied for the ministry at the University of Edinburgh; and was licensed as a preacher, and settled at Bressay, in Zetland, in 1809.

Here, in a new field—barren, indeed, to many—he began collecting zoophytes and the molluscs so profusely thrown on shore in that boreal region, and obtained the nucleus of perhaps the most extensive and perfect collection, illustrative of the natural history of the British Islands, in the possession of any private individual. In 1811 he was translated to the charge of Flisk, in Fifeshire, which he held for upwards of twenty years. In this retirement he had leisure, books, and, above all, the sympathies of a highly-accomplished wife, whose pencil was ever ready to illustrate the objects of his research. From the manse of Flisk he sent forth his first great work, the "Philosophy of Zoology," in two volumes. Its reception was most flattering to the author, and he was at once placed among the highest authorities in natural history. In a notice like the present it would be out of place to detail the bearing of this philosophic work on the views then entertained by the naturalists of Europe. Suffice it to say, that many theories were modified, and new views adopted, without much acknowledgement from whence the truth was obtained. In 1827 Dr Fleming published his "History of British Animals," which will ever be a monument of his patient and philosophic discrimination. Some idea may be formed of his labour, which was, indeed, one of love, by a glance at the number of genera and species, recent and extinct, found in the British Islands, therein described.

Mammalia, .	38 genera,	with	60 species.
Birds, . .	102	„	237 „
Reptiles, . .	7	„	12 „
Fishes, . .	89	„	170 „
Recent Mollusca,	153	„	597 „

And extinct Mollusca, no less than 1031 species.

All these were not only fully described, but the authority quoted and referred to, indicating an amount of labour and accuracy rarely met with. The "History of British Animals" is still a standard work, and formed the model for Forbes and Henley's beautiful work on the British Mollusca. In 1832 he accepted the presentation to the living of Clackmannan, which he held for two years, when he was appointed Professor of Natural Philosophy in King's College, Aberdeen. In this new field of labour he obtained the esteem and warm friendship of his fellow-professors, and the admiration of the students.

On the recommendation of Dr Chalmers, to whose enlightened views the founding of the chair of natural science in the New College of Edinburgh was mainly due, Dr Fleming was chosen first professor in the year 1845. This chair he occupied with eminent success until his lamented death. To this Journal Dr Fleming contributed nearly thirty original papers, all of the highest interest. His papers on the revolutions which have taken place in the animal kingdom, as indicated by geology, changed the views of many preceding writers, and his masterly criticism on Buckland's "Reliquia Diluviana" proved the Noachian deluge to be local, and to have left no traces on the strata. He also contributed many valuable articles to the "Encyclopædia Britannica," among which the most elaborate was on the Mollusca. When the Wernerian Society was in its prime, he was among its most active members, and contributed many of the best papers to be found in its Transactions. To the Royal Society he contributed also valuable geological papers, published in their Memoirs. Besides his two standard works already mentioned, he was the author of a work on the Seasons; and at the time of his death had just completed a small volume on the Lithology of Edinburgh. By the death of Dr Fleming, Natural History has lost one of its most earnest students and successful interpreters.

His love of truth and distrust of speculation were the marked features of his character. His knowledge of genera and species was, perhaps, with one exception (Milne-Edwards), the most extensive in Europe, and rendered him the best and sometimes the severest judge in condemning hasty generalization. A single sentence from one of his early papers portrays at once the principal feature of his scientific life,—"It is wiser to examine all the conditions of a problem before attempting its solution, than rashly suffer the imagination to indulge in speculation and conjecture."

Humboldt paid the well-deserved compliment to Robert Brown, the friend and contemporary of Dr Fleming, that he was "*facile princeps botanicorum*:" we can now apply, with all truth and earnestness, a similar tribute to our lamented friend, that he was unquestionably, for the last fifty years, "*facile princeps rerum naturalium indagator.*"

To this Journal Dr Fleming contributed the following original papers:—

1. On the Arctic and Skua Gulls.
2. On the *Sertularia gelatinosa* of Pallas.
3. On the Changes of Colours in the Feathers of Birds.
4. On a Fungus growing on Succinate of Ammonia.
5. On a Submarine Forest.
6. On the Revolution of the Animal Kingdom as indicated by Geognosy.
7. Gleanings of Natural History.
8. Do. do.
9. Do. do.
10. On the Distribution of British Animals.
11. Remarks on the Modern Strata.
12. On British Testaceous Annelides.
13. Remarks on the Deluge.
14. Remarks on the Evidence from the Animal Kingdom tending to prove that the Arctic Regions formerly enjoyed a milder Climate.
15. Additional Remarks on the Climate of Arctic Regions, in reply to Conybeare.
16. On the Remains of a Fossil Fish found in connection with a Bed of Coal at Clackmannan.
17. Description of a New Species of Skate.
18. On the Expediency of forming Harbours of Refuge between the Moray Frith and Frith of Forth.
19. On the recent Scottish Madrepores, with Remarks on the Climatic character of the Extinct Races.
20. Geological Notices.
21. On a Simple Form of Rain Gauge.
22. Remarks on the Calamite and Sternbergia.
23. On the Coal Plant termed Stigmaria.
24. On Rain Gauges.
25. On the Study of Natural History.
26. On Cedar-Wood Cabinets.
27. On the Means taken to Naturalize the Craw Fish in the South of Scotland.
28. On the Structure of Rocks.

Extract from an Obituary Notice of the late William C. Redfield, the American Meteorologist. By Professor WILLIAM B. ROGERS.—Mr Redfield, it is stated, was but little favoured in early life by opportunities of education. Even after his removal from his native state, Connecticut, to the city of New York, while yet a young man, he became immersed in business occupations such as are commonly thought incompatible with purely intellectual pursuits, and which in most cases leave but little leisure, and still less disposition, for the studies and investigations of science. But his strong inclination for scientific inquiries was not to be repressed by these discouragements, and he early enrolled himself among the active students of Meteorology, Physical Geography, and Geology.

In the first of these departments, which, it is well known, was the principal field of his investigations, his patience and sagacity in observing facts, and in collating and comparing the observations made by others, bore their rich fruit in that remarkable generalization which, under the title of the *Rotatory Theory of Storms*, is so commonly associated with his name. His earliest recognition of this law appears to have been suggested by the phenomena of the violent storm which in the year 1821 swept over New England; and it is not a little remarkable that it was a storm occurring the same year in Central Europe which led the German Meteorologists into a similar train of inquiry, and conducted Professor Dové, of Berlin, to a theory founded like that of Mr Redfield on the union of a progressive with a rotatory movement of the disturbed column of air.

It must not, however, be supposed that the fact of a revolving motion in some of the more violent storms had hitherto entirely escaped observation. Long before these systematic inquiries were thought of, navigators had recognised such a movement in some of the storms within the tropics. As far back as 1680, Captain Langford, in a paper on West Indian hurricanes, printed in the *Philosophical Transactions*, described them as progressive whirlwinds; and at the beginning of the present century, Colonel Capper, Mr Horsburgh, and a French writer, Romme, speak of the hurricanes or typhoons of the Indian and China seas as revolving storms. But these early observations and suggestions, pointing chiefly to local phenomena, and involving no clear conception of a general law, attracted little notice at the time of their publication, and were almost, if not entirely forgotten when Redfield and Dové, without a knowledge of each other's labours, framed the great generalization of the progressive-rotatory character of these atmospheric movements. Without detracting from Professor Dové's share in the investigation, it must, I think, be admitted that to Mr Redfield is pre-eminently due the credit of having first given to this law a truly inductive character; and I need hardly add, that his analysis, year after year, of the data diligently collected by him, was a work involving no small amount of detailed labour, as well as of sagacity and skill.

Although his investigations were directed principally to the storms of the Atlantic north of the Equator, he was early led on theoretical grounds to announce the proposition that in the Southern Hemisphere the motion of storms is the reverse of that presented by them in the Northern one, both as regards progression and rotation. This statement was soon after confirmed by Colonel Reid, the author of the well-known work on the *Law of Storms*, in an elaborate investigation of those of the Southern Indian Ocean.

These important generalizations, in the discovery and development of which Mr Redfield so largely shared, although not universally accepted either at home or abroad, have been adopted by most of those who have devoted themselves to the practical study of the subject, and in particular, have been advocated with much ability by Colonel Reid, already named, and by Mr Piddington, author of the *Sailor's Horn-Book for Storms*, to both of whom we are indebted for extensive researches in this branch of Meteorology. Through the treatises of these gentlemen, and the numerous memoirs of Mr Redfield, this theory is rapidly becoming familiar to the minds of navigators, many of whom have not only accepted but practically applied it. Even the general public have learned its language and its leading features, from the accounts of cyclones or revolving storms, so often repeated in the current news. It is but proper to add that the evidence in favour of this law has lately received an important accession from the publication by Mr Poey of Havana, of a tabular description of the gales of the West Indies and Atlantic, in which their progressive rotatory character, and the opposite directions of the movement on different sides of the equator, is shown by an investigation of between three and four hundred distinct storms, extending over a period of about the same number of years.

How far these laws are applicable to other than ocean storms, and what new laws or modifications of the rotatory principle may obtain in the interior of continents, are questions which do not seem at present capable of satisfactory answer. But however they may be decided by future investigations, we cannot, I think, fail to recognize in the generalizations of Redfield and his co-workers, a valuable contribution to positive knowledge, and an induction which, even should it be found strictly applicable only to the oceans and their coasts, is fraught with great practical good as well as scientific interest.

In saying this much, I would not be considered as accepting the theoretical views which Mr Redfield from time to time suggested in explanation of the origin of the revolving and progressive motion which he laboured to demonstrate. These speculations rarely put forth, and never very strenuously urged, appear to have had but little interest for him in comparison with the establishment of the *law of the phenomena.* Indeed, they were so briefly, and I must in candour add, so indistinctly presented, as to attract but little attention from the scientific world. At the same time it should be considered that even had Mr Redfield possessed a philosophical inventiveness, and a command of the exact sciences beyond what we would claim, or his own modest self-appreciation would admit as his, we could hardly have hoped that, in the present stage of investigation, he could have furnished a really satisfactory solution of the complex problem of the dynamics of storms. His labours, together with the concurring or the conflicting views of other Meteorologists at home and abroad, mark a great and beneficial progress in this difficult inquiry, and encourage the hope that, along with a knowledge of the laws of the winds, we shall hereafter be able to grasp in our thoughts the mode of their origin and the physical forces by which they are produced.

While giving his chief attention to the development of the Law of Storms, Mr Redfield found time for many useful observations in geology, especially in relation to the fossil fishes of the so-called New Red Sandstone belt of New Jersey and Connecticut, as well as those of the coal rocks of Eastern Virginia. In this inquiry, he had the valuable assistance of his son, Mr John H. Redfield, to whom we are indebted for descriptions and figures of several of these interesting fossils, as well as for important suggestions, founded on zoological affinities, as to the age of the belt of rocks in which they are entombed.

The continuation of this work had long, I believe, been a favourite plan with Mr Redfield, and seems to have been one of the last subjects connected with scientific pursuits which engaged his attention; for on his visit to Boston in the autumn, he spoke with much interest of having resumed the task of preparing, with the help, I think, of Professor Agassiz, a comprehensive monograph of the fossil fishes of this group of strata. But alas, on the 12th of February, he was called on to relinquish this and all other labours. He died at the age of 68 years, with faculties unimpaired, leaving us to regret that he could not have lived to continue his useful career, and yet giving us, in what he had done, cause to rejoice that he was permitted to work so long and so successfully in extending science and promoting the interests of mankind.

PUBLICATIONS RECEIVED.

L'Institut, July to November 1857.

Proceedings of the Royal Geographical Society of London. No xi.—*From the Society.*

Address at the Anniversary Meeting of the Royal Geographical Society, 25th May 1857. By Sir Roderick I. Murchison.—*From the Society.*

Journal of the United Service Institution, Whitehall Yard. No. 1, July 1857.—*From the Editor.*

Quarterly Journal of the Chemical Society. No. 39, for October 1857. —*From the Society.*

The Canadian Naturalist and Geologist, and Proceedings of the Natural History Society of Montreal for July and September 1857.—*From the Editors.*

Twenty-fourth Annual Report of the Royal Cornwall Polytechnic Society for 1856.—*From the Society.*

Handbook of Chemical Manipulation. By C. Greville Williams.—*From the Author.*

Goeppert, H. R. Ueber das Verhältniss der Boghead Parrot-Coal zur Steinkohle, 1857.—*From the Author.*

Philosophy of Theism, An Inquiry into the Dependence of Theism on Metaphysics, and the only possible way of arriving at a proof of the Existence of God. 1857.—*From the Author.*

Journal of the Asiatic Society of Bengal. No. III. for 1857.—*From the Editor.*

Catalogue of Human Crania in the Collection of the Academy of Sciences of Philadelphia. By J. Aitken Meigs, M.D.—*From the Society.*

Proceedings of the Academy of Science of Philadelphia from September 2, 1856, to March 31, 1857.—*From the Society.*

Description of Californian Fossils and Shells. By William P. Blake. —*From the Author.*

Transactions of the Academy of Sciences of St Louis. Vol. 1. 1857.—*From the Society.*

The following works have been sent by the Smithsonian Institution:—

Illustrations of Surface Geology. By Edward Hitchcock, LL.D.

Asteroid Supplement. By John D. Bunkle.

North American Oology. By Thos. M. Brewer, M.D. Part i. Raptores et Fissirostres.

Annual Report of the Board of Agents of the Smithsonian Institution for 1855 and 1856.

Appendix—Publications of Learned Societies and Periodicals in the Library of the Smithsonian Institution. Parts 1 and 2.

Observations on Mexican History and Archæology. By Brantz Mayer.

On the Recent Secular Period of the Aurora Borealis. By Denison Olmsted.

Appendix—Record of Auroral Phenomena. By Peter Force.

On the Relation of the Intensity of the Heat and Light of the Sun upon different Latitudes. By L. W. Meech.

Archæology of the United States. By Samuel F. Haven.

Researches on the Ammonia-Cobalt Bases. By Wolcott Gibbs and F. A. Garth.

Investigations, Chemical and Physiological, relative to certain American Vertebrata. By Joseph Jones, M.D.

The Tangencies of Circles and Spheres. By Benjamin Alvord.

New Tables for Determining Value of the Coefficients in the Perturbative Function of Planetary Motion. By John D. Bunkle.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Contributions to the Natural History of the Hudson's Bay Company's Territories. Part I.—*Rein-Deer.* By ANDREW MURRAY, Edinburgh.*

Perhaps I may be allowed, before proceeding to the proper subject of this paper, to say a few words in explanation of the somewhat ambitious title I have given to it, and of how I come to be in a position which entitles me, with a reasonable prospect of keeping the promise thereby implied, to offer the *first* part of "Contributions to the Natural History of Northern America."

In the Hudson's Bay Company's charter, which was granted by Charles II. in the year 1670, the preamble, or narrative of the cause why it was granted, bears that certain individuals had, "at their own great cost and charges, undertaken an expedition for Hudson's Bay, in the north-west part of America, for the discovery of a new passage into the South Sea, and for finding some trade for furs, minerals, and other considerable commodities, and by such their undertaking, have already made such discoveries as do encourage them to proceed further in pursuance of the said design, by means whereof there may probably arise very great advantage to us and our kingdom;" therefore his Majesty had resolved to grant them the tracts of land therein specified, and the sole trade and commerce thereof.

This, it will be seen, was no condition that the Company should do anything for science, or future expeditions, or discoveries. Whatever was the motive which led to the charter being granted, the grant itself was unfettered by any restriction or condition relating to such matters.

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* Communicated to the Royal Physical Society of Edinburgh, Nov. 25, 1857.

The Company, however, has always acted as if the motive which may have led to the grant, viz., the merit of past and the hope of future discoveries had imposed an express obligation on them to do everything in their power to foster researches in the dominions so conferred on them.

The extent to which the assistance of the Company has thus been given to science cannot be estimated; but it is not too much to say, that no public or private expedition was ever conducted through their territories which did not draw largely upon the liberality and assistance of the Company. Their own numerous explorations, their extensive geographical surveys, and the able and ready help which they have given to the search after Franklin and his crew, are instances which it is scarcely necessary to recal to the mind of the reader. I have, however, had special opportunity of seeing the liberal mode in which they extend their assistance to scientific objects, on the occasion of a botanical expedition being sent out a few years ago by an association formed in this city, to procure seeds of new and valuable hardy trees and plants from Oregon and the neighbouring districts. I acted as secretary to that association, and conducted the negotiations with the Hudson's Bay Company for securing their assistance to the collector (Mr Jeffrey). The liberal spirit in which I then found that the Company looked at things impressed me no less than the extent of the power they possessed. But there were other things which struck me with equal force. In studying the route followed by Jeffrey, I had the enormous extent of their territory forced strongly upon my attention—thousands of thousands of miles still inhabited only by the "wild;" and all this territory dotted over by the trading or hunting stations of the Company. I found also, in the occasional correspondence I had with the officers stationed at some of these remote posts, that they were obliging and intelligent. I imagined that many of them (from their hunting propensities, which may have led them to the life they followed) must have an instinctive taste for natural history; and when I put all this together, I felt that here was a great opportunity for enlarging our knowledge of the natural history of a considerable portion of the globe, which was lying fallow only because no one advanced his hand to seize it.

Seeing that no one else did so, I resolved to try what I could do myself, and I applied to the Governor and directors of the Hudson's Bay Company for permission to circulate throughout the posts scattered over their territory a paper which I prepared, entitled, "Instructions for Collecting Objects of Natural History;" in which, in few words, I gave general directions for collecting, preserving, and sending them home; and concluded by requesting those officers of the Hudson's Bay Company who might have a taste that way, to aid me by collecting for me, and transmitting to me the proceeds of their exertions.

Through the kind assistance of Mr Edward Ellice jun., my application was favourably received; and the Governor and directors not only sanctioned the distribution of my circulars, but charged themselves with it, and undertook to forward to me any collections that might be made,—the only condition imposed being, that the officers of the Company should not allow such collecting to interfere with the proper duties of their stations.

Five hundred copies of my instructions were accordingly sent out last year, and scattered over the length and breadth of the land; and the first-fruits of the seed so sown is the arrival, a few weeks ago, of six cases containing different objects of natural history, a portion of which will furnish the text for this paper.

I begin with the largest objects, viz., four magnificent heads and antlers of rein-deer, which have suggested some remarks on the disputed point of the identity of the European species with that of America, and on one or two other points of incidental interest.

The specimens received were sent by Mr Hargrave of York Factory. In his letter announcing their despatch, he says, —"Since writing the above, I have received from our trading station at Church-hill some specimens of "*Esquimaux*" rein-deer horns, obtained from the natives who visit that post from the south shores of Chesterfield Inlet,—two pairs of the handsomest of which, and two more from their very peculiar shape, I have caused to be bound into two bundles, under your address, and will ship them to London next month." It thus appears that the locality from which they come is that known as the Barren Ground, which is that sterile district forming the northernmost part of Canada, and bordering the shores of the Icy Sea, and that they belong to the variety described by Sir John Richardson under the name of "*Cervus Tarandus*, var. *a arctica* (Barren Ground caribou)." Whether that variety is or is not a distinct species is a question still open among naturalists. The weight of opinion certainly is in favour of its being merely a variety, and not a species. Sir John Richardson himself treats it as a variety; but at the same time he says (*Fauna Boreli-Americana*, vol. i.), "The rein-deer or caribou of North America are much less perfectly known (than the European). They have, indeed, so great a general resemblance in appearance and manners to the Lapland deer that they have always been considered to be the same species, without the fact having ever been completely established;" and again, in speaking of the two North American varieties which he describes,—viz., the Barren Ground caribou, and the Woodland caribou, he says, "Neither variety has as yet been properly compared with the European or Asiatic races of rein-deer, and the distinguishing characters, if any, are still unknown." Colonel Hamilton Smith, in Griffith's edition of Cuvier's *Animal Kingdom*, had previously spoken much in the same doubtful way, but still had not ventured to erect the varieties into species. He said, "The North American rein-deer or caribou are still very imperfectly known. There appear to be three varieties, one or more of which may actually form different species." The most recent evidence on the point, however, is that of Dr Gray, who, in his *Catalogue of the Specimens of Mammalia in the British Museum* (*Ungulata furcipeda*), 1852, has included the North American rein-deer, along with the Lapland rein-deer, under the old name of *Tarandus rangifer*, noticing them only as varieties. It does not matter whether we take this as an evidence of the views of naturalists in general at that date, or merely as the expression of the opinion of Dr Gray himself. No one ought to oppose the general opinion of concurrent naturalists, or the individual opinion of such a man as Dr Gray (admittedly one of our first living mammalogists), without at least distrusting his own judgment, and carefully weighing the arguments for and against the opinion which they have sanctioned by their authority; and it is only after having done so, to the best of my ability, that I have come to a different conclusion.

The grounds on which these naturalists retain the American as part of the European species are wholly negative. They do not find any differences sufficient to constitute specific characters. Let us, therefore, see what the differences in the characters of the two varieties really are, and examine their extent and value.

In the first place, the form of the horns is different. Sir John Richardson, indeed, by way of qualifying the value of this character, says, "It is to be recollected, however, that the antlers of the rein-deer assume an almost infinite number of forms, no two individuals having them alike." True; and the same may be said of the characters of all variable species; but in them, as in the rein-deer, there *is* a character of form which, constantly varying in individual detail, is constantly permanent in the general effect. The Lapland deer have one character, the North American another. Sir John Richardson gives figures of two heads of Barren Ground rein-deer, and although the minute details somewhat vary from those I received, the general effect is so much the same, that the figures of the one and the other might be taken for the first two heads sent to me by Mr Hargrave, one of which is figured below (fig. 1).

The most characteristic points in the American species are the triangular-bladed brow antler, the longer and more slender stem, and the fewer processes; but the first of these (the brow antler) is that on which I would chiefly rest, for it is a structure prepared and adapted to a condition of life, and therefore of more value as a specific character than any peculiarity not so adapted. In it the antler descends almost parallel to and close above the front, reaching down as far as the muzzle, there turning upwards in an abrupt, nearly straight line; the whole antler forming an elongated triangle, of which the apex is next the root of the horn. In the Lapland species the brow antler projects more directly out from the forehead, not being parallel to the front, but at a somewhat acute angle from it, and it is not formed in the triangular shape of the other, but, although palmated, has the ends curved up, as in the upper prongs or antlers (see fig. 2). Now, as

Fig. 1. (North American Species.)

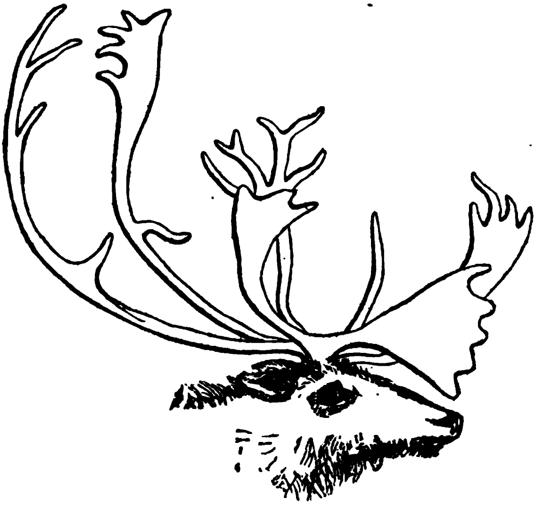
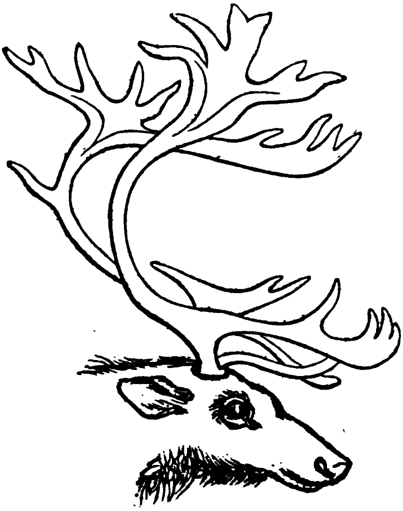


Fig. 2. (Lapland Species.)



already said, this character has more significance than the mere difference in form implies. We know that the deer with palmated horns are confined to the colder regions of the earth, and when the palmation is much developed it is probable that its purpose is to scrape and shovel away the snow from their food. But we see that all the deer with palmated horns are not equally provided with these shovels. Some are better and some worse; but none of them bears any comparison with the apparatus of the North American Barren Ground caribou. It has, in addition to the basal palmated triangular shovel, a second projecting prong with terminal points or fingers curved inwards, very like the brow antler of the Lapland deer. The use of these pieces of apparatus is sufficiently obvious. The upper projecting antler with curved points is to scrape into and break the surface of the hard crust of frozen snow; the triangular ploughshare or spade is to shovel away the softer snow below; and its structure is so admirably adapted for this purpose, that it is impossible to doubt the evidence of design exhibited in it. In the more perfect specimens the two projecting basal prongs fill up the whole space above the head, and the termination of the right prong is slightly curved towards the right, like a shovel, or an open hand looking in that direction, and the other is slightly curved in the opposite direction; so that, actually, we have a double-actioned shovel, no motion being lost—the reverse motion to the left, which was necessary to enable it to give the impetus of a fresh sweep to the right, clearing away in its course a shovelful to the left, and the returning motion to the right to give impetus to the motion to the left, shovelling away in its course a portion to the right. The less furnished specimens have only one single basal antler, but its straight upright position renders it nearly equally available for this double-actioned power.

The habits of this species also are known to correspond with this structure. Every author who treats of the North American species speaks of its using its horns to clear away the snow; and whether this was recorded or not, the well-used and much-worn state of the palmated divisions in the specimens now received proves sufficiently that this is their habit, and, by inference, that this is the purpose for which the peculiar form which these horns possess has been bestowed upon them. That the Lapland deer also use their horns, more or less, in removing the snow from the food which it covers, may be true; but that their horns are much less used for this purpose appears, not only from the form of the horns, but also from the notices of their habits, which we find in the works of those authors who have treated of them. In some of these a trivial notice occurs of their using their horns as well as their feet (which are their principal implements), but in most of them the feet are mentioned alone as used for this purpose, and no notice taken of the horns; so much so, that Colonel Smith says, in continuation of the passage already quoted,—"With them (the horns) they (the North American species) are also said to remove the snow, but it does not appear that this process has been noticed in Lapland." This flat triangular blade, therefore, which is the proper and full-grown form of horn in the adult animal, and thus the normal and specific form, I consider to be one of the principal characters of the North American species.

It may, however, be said that this habit, and corresponding apparatus, in the American rein-deer, are mere variations induced by climate, and not specific distinctions. But it humbly appears to me that this character cannot be so treated. That a species inhabiting a colder and more barren district should degenerate in size may be admitted; and we should not, on account of its smaller size, think of making it anything more than a climatal variety; but that an animal should be provided with a different or a more developed apparatus in order to accommodate it to a different condition of life, seems to imply much more than such a variety. If the North American Barren Ground animal is provided with this triangular spade or shovel because the snow is deeper in America than in Lapland, and a more efficient implement is necessary to enable it to get at its food, I look upon this as being in itself proof of the distinctness of the species. If, on the other hand, the snow is not deeper in America than in Lapland, then the difference in the apparatus makes still more against the climatal theory; for here we would have a different form for the same conditions of life.

There are other differences besides those of the horns. The colour of the North American species is lighter, both in its summer and its winter garb—being yellowish-brown or fawn-coloured, instead of dark-brown, in summer, and white, instead of grey, in winter—matters which *per se* are not of much consequence, but which, taken along with other differences, are of some weight.

Another most important point is, that the North American species has never been domesticated; but this involves a question much too long and too important to be fully discussed in the present communication. My own view of it is, that those social animals which are capable of being thoroughly domesticated are invariably found domesticated, and that the fact of an animal not being *domesticated* is proof that it is not *domesticable*. It may be said that it is the fault of the Esquimaux that the North American species are not domesticated, that they are a less intelligent race than the Laplanders, or that they have less aptitude for domesticating animals. But this is not the case. They have domesticated their Esquimaux dogs; and that they have tried to domesticate the rein-deer, and failed, is, I think, to be inferred from the following remark of Hearne:—"The moose is the easiest to tame and domesticate of any of the deer kind;" implying that the attempt had been made upon them all; and as we know from other sources, that the moose and other deer have been tamed, but never domesticated, the inference from this remark of Hearne's is, that if the North American species had been domesticable, they would have been domesticated by them. Mr Hutchins, indeed, speaking of the woodland caribou, says that several of the fawns had been brought up at the factories, and had become as *tame* as pet lambs; so have antelopes and deer of all kinds. But we must bear in mind that taming and domestication are two widely different things—a lion can be tamed, but not domesticated. Our common bull is domesticated, but often not tamed. The taming of a wild animal must thus not be confounded with the domestication of a social animal, and does not bear upon the point in question. Indeed, I firmly believe that this is not a matter which is left by nature to chance. How it is managed I do not pretend to say—possibly by an imperious instinctive desire impressed on the animal, craving that it should be domesticated, and compelling it to make the first advances; but whatever be the mode, I entertain no doubt that the securing the object has been carefully attended to by nature from the first; and where an animal is domesticable, there is as little chance of its being found undomesticated as there is of an undomesticable animal being found domesticated. The adoption of this (its domestication) as a specific character, would relieve our comparative anatomists and systematists from the inconsistencies and difficulties in which they have become involved in their attempts to determine the wild stocks from which our domesticated breeds have originally sprung. All inquiries on this subject have hitherto proceeded on the foregone conclusion that the domestic breeds must be referable to one or other of the wild species. Let this view be abandoned, and let it be conceded that it is at least possible that domesticable species have been created for the special use of man, and let the species, then, be compared with one another with as great a willingness to find them distinct as there hitherto has been a determination to find them the same, and I am sure that (in some of them at least) as good specific characters will be found for distinction as are thought sufficient in other species; and it must be kept in mind, that we are left, in considering the subject, almost entirely, if not wholly, to the *characters* of the animals themselves; for no instance occurs in which the actual period or process of domestication of any species has taken place under the eyes of man, or even has occurred within the period of authentic history. Neither can we point to any undisputed instance of a species having been once domesticated, and having afterwards relapsed into wildness. The African elephant, which we know from history was used, both in peace and war, by the Carthaginians and other North African nations in the time of the Romans, may be cited as an instance contradictory of this; but, in the first place, we do not know that the species possessed by the Carthaginians was the same species as that now found to the north of the equator in Africa, nor even that the species so found now is the same as the South African species. The effigies of some of the elephants represented on ancient Roman medals are no doubt figured with the large ears of the present South African species; but there may have been, and may still be, more than one species with large ears; and, in the second place, it is possible that there may be some species (among which the African elephant should possibly fall) which are only half domesticable,—such, perhaps, as our common duck (which has always a disposition to wander), the alpaco, &c., and which may not fall properly under the definition of domesticable animals, but rather form the link between those which are wholly so, and those which are not so at all. At the same time, I confess I prefer the undiluted theory, and hope at some future period to submit to the reader a more detailed explanation of my views and arguments on the subject.

Before leaving the horns, there is a statement made with regard to them by most authors which appears to me to call for revision, and regarding which I shall hope to get some of my new Hudson's Bay friends to make fresh observations. The statement is, that the male sheds his horns in November. Now, it appears to me so opposed to all the usual proceedings of nature that she should provide this admirable apparatus for clearing away the snow, only to throw it off at the very period when it would come into use, that I cannot bring myself to believe that there is not some error in the statement. I have therefore examined as many authorities as I could, in order to trace from whence this statement originated; for we often find in Natural History, that a statement originated by some one individual is repeated by subsequent writers without inquiry or consideration. The oldest statement on the subject which I find is that of Pennant in his "Arctic Zoology,"* where he says, "They go to rut in September, and the males *soon after* shed their horns." Hearne, who had ample opportunity of judging from personal observation, makes the following remarks in his journey to the Northern Ocean, 1795:†—"The month of October is the rutting season with the deer in these parts, and after the time of the courtship is over, the bucks separate from the does: the former proceed to the westward to take shelter in the woods during the winter, and the latter keep out

* Vol. i. p. 26.

† P. 197.

in the barren ground the whole year. This, though a general rule, is not without some exceptions, for I have frequently seen many does in the woods, though they bore no proportion to the number of bucks. This rule, therefore, only stands good respecting the deer to the north of Churchhill River; for the deer to the southward live promiscuously among the woods, as well as in the plains, and along the banks of rivers, lakes, &c., the whole year. The old buck-horns are very large, with many branches, and always drop off in the month of November, which is about the time they begin to approach the woods. This is undoubtedly wisely ordered by Providence, the better to enable them to escape from their enemies through the woods, otherwise they would become an easy prey to wolves and other beasts, and be liable to get entangled among the trees, even in ranging about in search of food. The same opinion may probably be admitted of the southern deer, which always reside among the woods, but the northern deer, though by far the smallest in this country, have much the largest horns, and the branches are so long, and at the same time spread so wide, as to make them more liable to be entangled among the underwoods than any other species of deer that I have noticed. The young bucks in those parts do not shed their horns so soon as the old ones. I have frequently seen them killed at or near Christmas, and could discover no appearance of their horns being loose. The does do not shed their horns till the summer, so that when the buck's horns are ready to drop off, the horns of the does are all hairy, and scarcely come to their full growth." This certainly is the testimony of a man apparently conscientious and desirous to tell the truth, with no object to do otherwise, and, moreover, with ample opportunity of getting at the truth, and with his attention specially directed to the subject, all which of course make the matter only more embarrassing. Next comes Colonel Smith: "*The males drop their horns after the rutting season in November*, but the females, if gravid, keep theirs till May; under other circumstances, they drop theirs at the same time with the males; the new ones are eight months growing, not being complete till August." The anomaly to which I am alluding appears, however, to have struck him as well as Hearn, for he offers the following explanation of the rein-deer shedding its horns so early as November:—" The horns of the rein-deer, indeed, drop in winter, but this takes place only at a period when the snow is already not only very deep, but frozen hard, and even then we see that the females, when gravid, and therefore in want of a greater supply of food, preserve theirs till May."* Of the two, I must say I prefer Hearne's reason for the horns dropping in November. The harder frozen the snow, the more need of good implements to get at their food, which is under it; and if it is necessary for the females getting their food that they should retain their horns through the winter, the additional claim arising from their bearing an embryo or a fœtus scarcely seems sufficient to account for their having the means of securing it, while the males have not. Another, and not the least formidable testimony, is that of Sir John Richardson.† He says—" This (the velvety covering of the horns peeling off) takes place in September, previous to the commencement of the rutting season, *and by the end of November most of the old bucks have shed their horns.* The young males retain theirs much longer, and the females do not lose their horns until they are about to drop their young, in the month of May." Now, Sir John had a good opportunity of ascertaining how the fact stood; but I do not wholly read the paragraph I have quoted as a statement depending upon his own personal observation, for he goes on—" Hearne observes that the Barren Ground caribou bears horns twice the size of those of the woodland variety, notwithstanding that the latter was a much larger animal;"—thus showing that at the very time he wrote the paragraph he had been consulting Hearne, and it is just possible that it is his (Hearne's) observation that he is repeating, instead of giving the results of his own. His statement of the movements of the rein-deer is more important, and it corresponds more with Hearne's view of the reason why the horns are shed in November. He says‡—" The Barren Ground caribou, which resort to the coast of the Arctic Sea in summer, retire in winter to the woods lying between the sixty-third and sixty-sixth degree of latitude, where they feed on the *Usneæ*, *Alectoriæ*, and other lichens which hang from the trees, and on the long grass of the swamps. About the end of April, when the partial melting of the snow has softened the *Cetrariæ*, *Cornicularia*, and *Cenomyces*, which clothe the Barren Grounds like a carpet, they make short excursions from the woods, but return to them when the weather is frosty. In May the females proceed to the sea-coast, and towards the end of June the males are in full march in the same direction. At that period the power of the sun has dried up the lichens on the Barren Grounds, and the caribou frequent the moist pastures which cover the bottoms of the narrow valleys on the coasts and islands of the Arctic Sea, where they graze on the sprouting carices and on the withered grass or hay of the preceding year, which is at that period still standing and retaining part of its sap. Their spring journey is performed partly on the snow, and partly, after the snow has disappeared, on the ice covering the rivers and lakes, which have in general a northerly direction. Soon after their arrival on the coast the females drop their young; they commence their return to the south in September, and reach the vicinity of the woods towards the end of October, where they are joined by the males. This journey takes place after the snow has fallen, and they scrape it away with their feet to procure the lichens, which are then tender and pulpy, being preserved moist and unfrozen by the heat still remaining in the earth." "The lichens on which the caribou principally feed whilst on the Barren Grounds, are the *Cornicularia tristis*, *divergens*, and *ochroleuca*, the *Cetraria nivalis*, *cucullata*, and *islandica*, and the *Cenomyce rangiferina*,"*—all low ground-growing species. The statements, however, of the latest observer on the subject, Dr Armstrong†, are somewhat different, both as regards the shedding of the horns and the migration of the deer. As to the first, he says, "The calving season, as far as my observation enables me to judge, is *in June, prior to, and coeval with which the bucks shed their antlers*, which appear to be again entirely reproduced in the latter end of August and early in September;" and elsewhere he especially notices the rapidity of growth of the new horns. As regards the second part, he makes the following remarks; and observations to the same effect occur in "Osborn's Voyage of the Investigator:"—"It has hitherto been the generally received opinion that these animals migrate to the southward, on the approach of winter, to lands where the cold is less intense and the pasturage more abundant, an opinion formed from the writings of distinguished Polar voyagers who formerly wintered amid the icy solitudes of the North; but the experience of four winters enables me to speak from the result of observations in contradistinction to this. In the Prince of Wales' Strait rein-deer were seen in January—our distant position from the shore not enabling us to hunt during the winter; and in the Bay of Mercy, for two successive winters, they were constant inhabitants of the land, and were killed throughout the winter months of the coldest season in the records of arctic voyaging. How far the migratory habits of the animal may be established in a more southern latitude on the coast of America, in their instinctive resort to localities where pasturage may be more abundant, I shall not attempt to decide; but this I will say, that from the more distant lands of the Polar Sea they do not migrate on the approach of winter, but remain there constant inhabitants. I have remarked, however, that as the season of thaw sets in (May and June), coeval with the calving of the does, these generally resort to the ravines and valleys bordering the coast, where the pasturage is so much more abundant."*

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* Griffith's Cuvier's Animal Kingdom, vol. iv., p. 70.

† Fauna Bor. Am. i., p. 241.

‡ Loc. cit., p. 242.

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* Loc. cit., p. 243.

† Personal Narrative of the Discovery of the North-west Passage, 1857.

These narratives of the habits and food of the animal at different periods, and in different regions, are sufficiently discordant to induce us to pause before coming to an opinion upon them. They show the necessity of further observations, and indicate the points to which attention should be directed. Their tendency, on the whole, however, is in favour of what appears to me the necessary inference to be drawn from the horns. To the statements of the foregoing authors, where opposed to this view, I reply by pointing to the horns themselves. Not only is the ploughshare there, but it is evident it has been much and hard used; the edges are all rubbed off, and the inequalities smoothed down; and it is plain that this cannot have been done by removing snow in the summer-time, when it is all melted. From the specimens I have received I draw the following inferences:—*1st*, That they are the heads of old bucks: the size of the horns and worn teeth prove this; *2d*, That the triangular palmated plates on their horns are formed and used for the purpose of shovelling away the snow to get at their food; *3d*, That they have been used for this. *4th*, That they have been so used for a longer period than the month or six weeks after snow has fallen (in September and October) which Sir John Richardson gives them for returning over the Barren Grounds, where the lichens grow which they disinter for their food; *5th*, That it is in the winter they have been so rubbed and worn, and not in the summer; and, *lastly*, It should follow from these premises that the horns are not shed in November. Another argument against their being shed then may be drawn from what takes place with other deer. The red deer, for instance, in this country has its rutting season in September (the same time as the rein-deer), and the horns are not shed till April or May—the oldest, however, shedding them first. It is to be kept in mind that the rutting season and the growth of the horns are intimately connected together, the reproducing power under which the new horns advance in growth being then exerted to the utmost. The other North American deer, like the red deer and other stags, do not shed their horns before winter. The moose keeps them the whole winter; and the instance in question, if true, seems to be a solitary exception to the economy of all the rest of the deer tribe, so far as I have been able to ascertain. Still, the statements on the subject are too explicit, and from too high authority, to be evaded by an argument or an inference; although I must say that it is long since I have been of opinion that circumstantial evidence is of ten times more value than the best direct testimony in the world. All that I mean, therefore, by making these remarks, is to invite the attention of those who may have the opportunity of observing the animals to a more careful examination of the economy of the old bucks in respect to the shedding of their horns.

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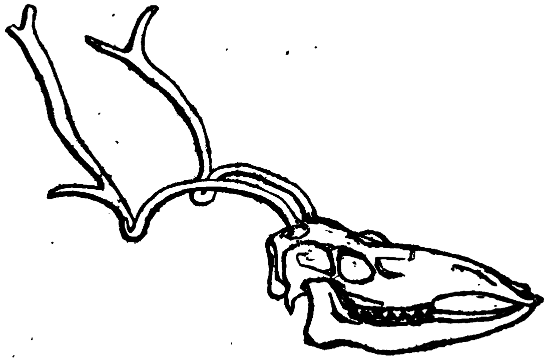
* Loc. cit., p. 276.

The two smaller heads sent me by Mr Hargrave as exceptional, from the form of their horns, are interesting. The one, from the state of its worn teeth, is obviously an old deer, although small in size, and with small horns. Its horns have, however, met with a distortion by which they have a curious bend in the middle, as shown in this figure. The cause, whatever it may have been, has affected them both equally, which is not usually the case where horns are distorted—it generally happening that if one horn is injured so that it takes reduced dimensions, the nourishment which was meant for it is diverted to the other horn; and we have the two horns characterized, one by defect, and the other by excess. It is not easy to say what may have been the cause of this curious distortion. It may be that the poor animal, when its horns were still soft and young, got entangled among brushwood; and that here is the silent evidence of long struggles on the part of the animal, and of perhaps days of famine, before it succeeded in freeing itself from the bonds which held it. Or it may merely be a distortion consequent upon the old age of the animal, for we often find the horns in old deer stunted and distorted, although it is not usual to find them so symmetrically disfigured. It will be observed that this head wants the triangular ploughshare in front, but as it is obviously an abnormal and exceptional head, this want goes for nothing in the question of species. One of the other heads sent by Mr Hargrave is a young one, as shown by the teeth, and has not yet got the fan-shaped ploughshare, which, like

Fig. 3.



other antlers, only appears after the animal has acquired a certain age. It is unnecessary, moreover, to say, that in the observations I have previously made as to the form of the horns in the different species, I have spoken of characteristic examples of the full-grown animal, not of young or exceptional horns.

The dentition in the young deer is deserving of notice. The incisors overlap one another in a curious manner, except the outermost, which fits into a groove on the edge of the penultimate tooth. In the older heads the teeth stand apart. They are all very small; and the mode in which they are worn away in the older animals is peculiar. Instead of being worn flat on the crown, or somewhat inwards, as is the case with other ruminant animals, the front of the central teeth are worn down obliquely outwards. This arises most certainly, not from nipping *Usneas* hanging from the trees, or from cropping grass like a sheep, but from grubbing up the *Cenomyces* and other lichens growing flat on the surface of the ground—an additional argument in favour of these being their principal food.

Another interesting structure in these animals remains to be noticed; I mean the fur or hair. Of this Sir John Richardson says—"In the month of July the caribou sheds its winter covering, and acquires a short smooth coat of hair of a colour composed of clove brown, mingled with deep reddish and yellowish browns; the under surface of the neck, the belly, and the inner sides of the extremities remaining white in all seasons. The hair at first is fine and flexible, but as it lengthens it increases gradually in diameter at its roots, becoming at the same time white, soft, and compressible, and brittle, like the hair of the moose deer. In the course of the winter the thickness of the hairs at their roots becomes so great that they are exceedingly close, and no longer lie down smoothly, but stand erect; and they are then so soft below that the flexible coloured points are easily rubbed off, and the fur appears white, especially on the flanks. The closeness of the hair of the caribou, and the lightness of its skin when properly dressed, renders it the most appropriate article for winter clothing in the high latitudes. The skins of the young deer make the best dresses, and they should be killed for that purpose in the months of August or September, as after the latter date the hair becomes too long and brittle. The prime parts of eight or ten skins make a complete suit of clothing for a grown person, which is so impervious to the cold, that with the addition of a blanket of the same material, any one so clothed may bivouac on the snow with safety, and even with comfort, in the most intense cold of an arctic winter's night."*

On a close examination of the skin, I have not found anything particularly different from the skin of any other animal. The hair is more patent to examination, and is interesting, not only in relation to its own economy, but also in relation to the views held by histologists of the structure of hair in general, and by physiologists of its mode of growth and developement. It has already been made known by Professor Busk, that the hair of the deer tribe is peculiar, being almost entirely cellular; and the hair has been described and figured by Dr Inman, in an able paper "On the Natural History and Microscopic Character of Hair," published in the "Proceedings of the Literary and Philosophical Society of Liverpool," No. 7 (1851 to 1853); but as my observation somewhat differs from his, and he has limited his figure to what appears to me an inaccurate representation of the larger hair in one aspect, and has not described the equally interesting finer and smaller hairs, I have thought it desirable to give a careful view of both, with magnified representations of different sections; and that there may be no exception taken to their accuracy, I have got the drawing made by Dr Greville, whose name is a sufficient guarantee for its fidelity. The subject figured is the skin and hairs of one of the above-mentioned North American rein-deer; but the structure seems to be the same in all deer—at least it is so in all which I have examined—in the moose in the red-deer, roe-deer, musk-deer, &c., but not in the antelopes.

The figure on the right hand represents a somewhat magnified portion of the skin, with both kinds of hair issuing from it; the left hand figure represents a more highly magnified small hair; the upper centre figure shows a highly magnified portion of the large hair; the lower centre figure a transverse section of this; and the middle centre a longitudinal section.

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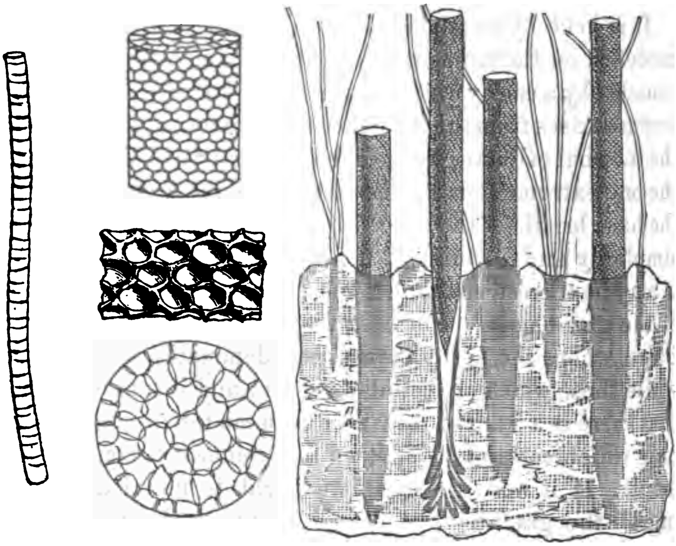
* Loc. cit., p. 242.

Fig. 4.



Dr Inman says:* "In the deer the cells are so numerous as to occupy the whole of the body of the hair, and so irregular that no particular place of subdivision can be traced;" and his figure quite corresponds with this, the cells being there shown as amorphous; but it will be seen from the above figure that they are truly polygonal—for the most part hexagonal, and there are very distinct septa and lines of separation. In fact, as Dr Greville pointed out to me, one of the most striking points in this structure is its close resemblance to (I might almost say identity with) polygonal cellular tissue seen in the hairs and other parts of plants.

The difference between the long and thick hairs, and the fine small hairs, is interesting and suggestive. We have here types of the two great sections into which hair may be divided growing side by side; the one wholly cellular, the other apparently without cells at all, and wholly horny and cortical. I do not doubt that, by the use of proper agents, we would find that the latter has a central cellular medulla or pith, as in the human and other hairs of a similar appearance. Like them, and most other hairs of that texture also, these fine hairs are imbricated, as may be faintly seen in the woodcut.

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* Loc. cit., p. 89.

It is held by physiologists that both these kinds of hair are modelled on the same plan, viz., that of a cellular interior, surrounded by a horny cortical exterior, and that the difference in texture arises from the difference in the extent of development of the internal cellular pith or of the external cortical covering. In the one extreme forming the soft hair of the deer; in the other, the hard bristle of the sow. This view recommends itself by its simplicity and the unity of the *modus operandi;* but although it may be correct, so far as it goes, it does not explain the whole of the phenomena. For example, it does not explain why the hairs, where the horny covering predominates, are imbricated while those which are cellular are not; and it is to be observed, that there is a want of transition between the two characters of hair which certainly is opposed to a common mode of development. If it were the same, we ought to find hairs exhibiting all the gradations of passage between the two extremes, which we do not. Furthermore, they appeared to be designed for different purposes. Speaking in a general way, the horny or bristly hair is characteristic either of carnivorous animals, who have a greater supply of caloric than vegetable feeders, or of graminivorous animals inhabiting warm climates; while the cellular hairs in question are confined to the deer tribe, most of whom inhabit cold climates. It has usually been said, that the fine hair found at the roots of the coarser hair in these animals is an additional provision of nature for the warmth of the animal. It rather appears to me that in the deer at least it is the larger cellular hairs which have been added for this purpose (no one can look at them, I think, without seeing how admirably they are adapted for this), and that the horny hairs, whose office may possibly be as much that of a regulator of temperature as of a heating apparatus, are the normal hairs of the animal reduced to the smallest dimensions. If these two kinds of hair have distinct functions, their mode of development may also possess distinctive characters. We see that their roots extend to very different depths in the skin, and although we know that the hair is a mere appendage of the skin, produced by its involution or evolution, it may be that, by drawing more of its substance from one layer than from another, the differences in its appearance, which we have been considering, are produced. These are points on which the recent researches of Kölliker, Leydig, Queckett, Inman, and other microscopists have not touched. It is only a skilful histologist who can take them up with any chance of success; and as I have no pretensions to such a title, I am glad to have enlisted my friend Dr Turner (Demonstrator of Anatomy) in the examination of the subject, and he has undertaken to see if he can throw any further light upon it.

Another interesting provision with regard to the hair is, that in the rein-deer and the moose or elk (the only two arctic species or families) the part of the muzzle called the muffle, instead of being left bare and moist, as in other ruminants, is clothed with hair—this forming the generic character of the group. A moment's consideration of what the effect would be of plunging a bare and moist muzzle into frozen snow, in the search after lichens, will show how necessary a deviation this is from the normal structure of that part. At first sight one might expect, on like grounds, some analogous deviation from the normal condition of the stomach in arctic animals, but there is none such, and the reason probably is that that organ is not very sensitive, and any special protection to it against the coldness of the food is therefore unnecessary.

The skin appears to be a good deal cut up before winter by the gad-flies and *Œstri*, and we have no account how the damage done by these creatures is repaired before the severity of the winter begins to be felt; doubtless, the sores quickly heal as soon as the originators of the mischief drop out, and the part will only be thicker on account of the healing process; so that it would be rather curious if the unattacked part of the skin turned out to be in reality the weakest. The hair, too, is cast and replaced at this time, so that the comfort of the animals is sufficiently provided for.

On the Polarized Condition of the Muscular and of the Nervous Tissue in the Living or Recently Killed Animal: —Muscular Force and Nerve Force, POLAR *Forces.* By H. F. BAXTER, Esq.

Having arrived at the conclusion, in a previous paper,* that the muscular and nervous tissues are, during life, in a peculiar state or condition, which has been termed *polarized*, the following question naturally arises,—Can this state, dependent as it evidently is upon *nutrition*, be *increased* by any artificial means? That it may be diminished or easily destroyed is to be inferred from the fact, that whatever interferes with the proper nutrition of a muscle or nerve, or disorganizes their structure, whether by mechanical or chemical agencies, destroys also the conditions upon which the *existence* of the muscular or nerve currents depends; and it is, it may be observed, from the manifestation of these currents that the existence of this *polarized* condition is inferred. It is reasonable, therefore, to suppose, that it might be by the employment of the *electric force* (or *current*) that we should perhaps obtain some evidence to assist in solving this problem.

In considering the question of the influence of electricity upon the muscular and nervous tissues, we are necessarily brought to the examination of the various experiments that have been undertaken from the period of Galvani's celebrated discovery up to the present time, in which *muscular contraction* has been induced by means of the agency of electricity upon the nerves. On the present occasion, however, as it is by means of the *galvanometer* rather than by *muscular contraction* alone that the evidence proper for the solution of our question might be obtained, it will not be necessary to enter into a critical review of the various results made known to us by our predecessors; but those facts may be stated in the form of a few propositions which appear to have been well-established by the labours of Volta, Marianini, Nobili, Matteucci, Marshall Hall, Du Bois Reymond, and others,* and to which we shall have occasion to allude in the course of the inquiry.

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* Edinburgh New Philosophical Journal, New Series, January 1858.

1*st*, When an electric current traverses a nerve, it is only at the *opening* and *closing* of the circuit that *muscular contraction* ensues.

2dly, No *muscular contraction* occurs *during* the passage of the current.

3dly, After the *inverse* current has passed for some time along a nerve, upon opening the circuit, *tetanic contractions* are produced; with the *direct* current *no contraction* takes place.

The question may now arise,—Can the *muscular contraction* of a limb be considered as evidence of an *increase of the polarized* state or condition of the nerve going to that limb? Previous to considering this question, let us endeavour to ascertain whether any increase occurs in the muscular or the nerve current under these circumstances.

MARIANINI,† under the supposition that electricity, in these experiments, accumulated in the tissues, says,—" En appliquant avec soin les fils du galvanomètre aux fibres palpitantes ou aux nerfs adhérens, on pourrait, peut-être, détourner en partie ces courans, et les faire passer par le galvanomètre; mais les expériences que j'ai faites jusqu' à présent sur ce point aussi délicat ne me permettent pas encore de rien affirmer avec assurance."

MATTEUCCI‡ adds, " How is this tetanic action produced? It is easy to convince oneself, if any doubt could be entertained upon the subject, that there is no electricity rendered latent either in the nerves or in the muscles by the passage of the inverse current. My endeavours to discover signs of any, by the aid of the condenser, have been entirely fruitless. Likewise there are no signs, on opening the circuit, of any electric current in circulation. I have made myself quite certain of this fact by means of the galvanometer, employing at the same time a pile of tetanized frogs."

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* In BECQUEREL's Traité de l'Electricité, will be found an account of the views of the earlier inquirers, and also some valuable observations of his own in regard to animal electricity.

† Annales de Chimie et de Physique, tom. lvi., p. 387, 1834.

‡ Phil. Trans., 1846, part ii., p. 236.

In the following experiments, the current either from 1, 3, or 6 of Grove's middling-sized cells was passed through a detached muscle, or a portion of the sciatic nerve, the platinum electrodes being so arranged that the *anode* or *platinum* extremity was in contact with the *base* or *transverse* section of the fibre, and the *cathode* or *zinc* extremity in contact with the *external* or *longitudinal* surface; the *direction* of the current being in accordance with the normal direction of the muscular or nerve current, or that which is called the *inverse* current. The muscle or nerve, which was from rabbits, guinea-pigs, and frogs, was placed upon two pieces of glass, separated from each other, so that the current should traverse the substance of the tissue; but how far it was conducted by the surface and not by the fibre alone, may be a question difficult to decide.

The normal effect of the muscular or nerve current was first ascertained and noted; the former amounting to 4° or 5°, the latter to 2° or 3°, depending, however, upon the state of the nerve or muscle, and also upon the animal. The current from the battery was allowed to pass along the fibre for different periods of time, when the effect upon the muscular or nerve current was then examined.

The effects, generally speaking, were as follow:—

With the current from *one* cell, after it had passed five minutes, the amount both of the muscular and nerve current was slightly diminished; after ten minutes, the nerve current was not obtained, the muscular current still existed; fifteen minutes, no nerve current, nerve dry, muscular current but slight; twenty minutes, no effect; twenty-five minutes, no effect.

With the current from *three* cells, after five minutes, the nerve current very slight, the muscular current somewhat diminished; after ten minutes, no nerve current, nerve dry, muscular current slight; fifteen minutes, no effect from nerve, muscular current very slight; twenty minutes, no effect from either; twenty-five minutes, no effect.

With the current from *six* cells, after five minutes, no nerve current, nerve dry, muscular current very slight; after ten minutes, indefinite indications of the needle with the nerve, no muscular current; fifteen minutes, no effect with nerve, with the muscle the current occasionally indicated a reversed direction; twenty minutes, no effect with the nerve, indefinite indications of the needle with the muscle; twenty-five minutes, effects similar to those last observed.

As no distinct evidence of an *increase* either in the muscular or nerve current was obtained, it has not been thought necessary to particularize the results. Great care was taken in all these experiments to depolarize the platinum electrodes after the completion of each circuit, and to have them well cleaned.

In the next series of experiments, the current was passed *direct*, so as to traverse the fibre in the contrary direction to that of the muscular or nerve current. As the results did not indicate any *increase*, or even any decided *diminution* in the muscular or nerve current, and were in many respects similar to those we have already related with the *inverse* current, they need not be detailed.

Employing litmus paper to test the indications of the two surfaces of the fibre in contact with the electrodes, the effects were the same; there was no distinct acid or alkaline deposit on either of the two surfaces. The tissues soon became dry.

The results in all these experiments, after the current had passed for some time through the nerve or muscle, and especially when more than one cell was employed, evidently arose from the electro-chemical actions set up in the moist animal substances, by means of the electric current employed destroying the conditions necessary for the existence of the muscular or nerve current, and setting up new actions. It is extremely doubtful whether the current *traversed* the fibre;[*] it might have been conducted by the surface alone; and, in considering the action of the current upon the tissue, it is not difficult to perceive that its effect (its mode of action) would not be that of *increasing* the electrical state of the tissues, but more of a *disorganizing* action. What is evidently required is, to *induce* the *negative* electrical state of the fibre, which constitutes its *polarized* condition; and it does not appear that, by merely passing a current through it, we should be able to produce the effect we wish.

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[*] Matteucci believes that the current is conducted by the liquid part of the nerve. Phil. Trans. 1850, p. i., p. 288.

Instead of a *constant*, an *intermitting* current, from an ordinary medical electro-magnetic machine, was employed, and made to traverse the muscular or nerve fibre as before. In these instances, there was no indication of an *increase* in either of the muscular or nerve currents.

The limbs of a galvanoscopic frog were now placed in separate glass vessels, employing the current from the battery of six cells, the current being *inverse* in one limb and *direct* in the other, as in Matteucci's experiment. When *tetanic contractions* were produced in the *inverse* limb, an attempt was made to ascertain whether any difference existed between the muscular and nerve currents of both limbs. No decided difference between the two limbs could be detected; differences were occasionally observed, but the nerve current in the *direct* limb was apparently as frequently *increased* as that of the *inverse* limb, but the muscular and nerve currents of both limbs, and in other parts of the same frog, generally indicated a greater amount of deflection previous to the passage of the current from the battery than afterwards.

It may be asked, Do not these *tetanic contractions* indicate an *increase* in the polarized state or condition of the nerve? Du Bois Reymond* states, that he has obtained indications of an *increase* in the nerve current, by passing an electric current along a portion of the same nerve. He says, "If any part of a nerve is submitted to the action of a permanent current, the nerve, in its whole extent, suddenly undergoes a material change in its internal constitution, which disappears on breaking the circuit as suddenly as it came on. This change, which is called the electro-tonic state, is evidenced by a new electro-motive power, which every point of the whole length of the nerve acquires during the passage of the current, so as to produce, in addition to the nerve current, a current in the direction of the extrinsic current. As regards this new mode of action, the nerve may be compared to a voltaic pile, and the transverse section loses its essential import. Hence the electric effects of the nerve, when in the electro-tonic state, may also be observed in nerves without previously dividing them."

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*On Animal Electricity. Edited by H. Bence Jones, M.D., p. 213.

The experiment was repeated in the following manner:—Platinum wires, connected with the galvanometer, were placed in contact with the two portions of the nerve (the *longitudinal* and the *transverse* sections), so as to obtain the nerve current. The current from one, three, or six of Grove's middling-sized cells was then passed along another portion of the nerve, at different distances from that portion connected with the galvanometer, the electrodes of the battery being pointed. When the current from the battery was confined to a small portion of the nerve, and at some distance (an inch or more) from the other portion, it very rarely happened that we could obtain any effect upon the nerve current. When the electrodes of the galvanometer comprised half-an-inch of the nerve, and the electrodes of the battery also the same extent, and were not far from those connected with the galvanometer, then an effect was frequently produced upon the nerve current; the effect, however, was, generally speaking, that of a *decrease* in the nerve current, and this took place whatever might be the direction of the current from the battery, whether coinciding with, or in opposition to, the direction of the nerve current. It very seldom happened that an *increase* in the nerve current was obtained; and, as these results were chiefly observed to occur when more than one cell was employed, the effects—the apparent *increase*, and perhaps the *decrease*, of the nerve current—may be more correctly referred to some disturbance in the position of that portion of the nerve between the electrodes of the galvanometer, arising during the passage of the current in the other portion; an *increase* occurring when the nerve pressed against the electrodes, and a *decrease* when separated from them. It may be just remarked that the nerves soon became dry in these experiments.

The nerves were taken from the frog, guinea-pig, and rabbit.

Similar results were obtained when muscles were employed in the same manner for the purpose of ascertaining the effect upon the muscular current.

Although the effects observed may partly arise from an alteration in the contacts of the two surfaces of the nerve between the electrodes of the galvanometer, we nevertheless believe, with Du Bois Reymond, that the passage of an electric current in another portion of the same nerve is capable of affecting the conditions upon which the nerve current depends; but we have not been so fortunate as Du Bois Reymond in obtaining such decided and definite evidence as could have been wished in regard to the *increase* of the nerve current. In all these experiments the *distance* of the electrodes, both of the galvanometer and battery, from each other, and the *extent* of the nerve between each of the electrodes, and also the state of the nerve in regard to the dryness of its surface, are points of the utmost importance to be considered and attended to in judging of the final result.

The only conclusions that can be deduced from the foregoing investigations, contained in the former as well as present paper, are the following:—

1*st.* That we have no evidence of being able to *increase* the *polarized* condition of the nervous and of the muscular tissue by artificial means, such as the electric current; but it is highly probable.

2d. That an *increase* of this *polarized* condition may arise from an increased action of those changes which take place in the living animal, such as *nutrition,* being the same means by which it is produced and maintained in the living animal.

Before acceding to these conclusions, it may be reasonably asked, Have we not other evidence besides that afforded by means of the galvanometer to indicate an *increase* in the polarized condition of the nerve? Do not the *tetanic contractions* which are observed in a limb whose nerve has been subjected to the action of an electric current (*inverse*), indicate an *increased* action of the nerve? Previous to discussing this question, which will be considered in the concluding remarks, the following experiment was performed:—

A current from six of Grove's cells was passed through the limb of a galvanoscopic frog in the *inverse* direction, and as soon as *tetanic contractions* were produced, the nerve was divided at the junction of the nerve with the muscles of the limb; the tetanic contractions ceased. The two ends of the divided nerve were now placed in apposition, but no tetanic contractions ensued. This *inverse* current was again allowed to pass for some time through the nerve thus united, but no tetanic contractions occurred upon the breaking of the circuit. Great care, however, is required in this experiment to divide the nerve at the exact point where it emerges from the muscles, as pointed out by Matteucci, otherwise the tetantic contractions take place.

The results of this experiment only tend to confirm what has been already satisfactorily proved by others, that the *continuity* of the nerve fibre in the nerve leading to the muscle, is necessary for the conduction of the impression excited at the distal end of the nerve in order to arouse muscular contraction. It need scarcely be added, that the muscular and nerve currents may, however, be obtained under these circumstances between the separated portions.

Concluding Remarks.

The results of our recent investigations having led to the conclusion that the muscular and the nervous tissues are both, during life, in a *polarized* state or condition, and from our inability to *increase* this state by any artificial means, it being produced and maintained by *nutrition*, would almost stamp it as being peculiar to the organic kingdom. All the experiments tend, moreover, to show that it must be by acting upon and employing the means by which it is produced and maintained, viz., through the act of nutrition, that we can hope to succeed; for it is reasonable to suppose, that whatever would increase this act would also increase this condition, as shown by an increase in the muscular and nerve currents under these circumstances. That *tetanic contractions* may be produced by means of the electric current upon the nerve, might perhaps be adduced as an argument in favour of the supposition that an increased action of the nerves is produced, and, consequently, an *increase* in the polarized condition; but how far the peculiar state of the nerve which produces *tetanic contractions* under these circumstances is due to an increase of the normal *polarized* condition is open to remark, as we shall endeavour to point out.

When an electric current from a voltaic circle or any other source traverses a nerve or muscle, the muscle or nerve forms a part of the conductor, as any other moist substance or fluid (electrolyte) might do, and becomes polarized; the polarized condition of the muscle or nerve under these circumstances being manifested, as the other points of the circuit, in the *longitudinal* direction, one extremity being positive or negative to the other; and when the electrodes of a galvanometer are applied, one to each end, an electric current is manifested. Nothing of this sort, however, is manifested in the normal polarized condition of the muscular or nerve fibre as it exists in the animal body; the polar action, as then manifested by the galvanometer, is more in the *transverse* direction, or, more correctly speaking, the tissue is in one electric state *negative*, and the *positive* state is external to it either in the sarcolemma or neurilemma or in the blood. Hence we see the important difference between these two modes or conditions or states. To suppose, therefore, that an electric current, in traversing a muscle or nerve, increases its normal polarized condition, would only lead to erroneous ideas of the subject; it may, however, induce a change, under certain circumstances, in the condition of the nerve, tantamount to an increased action of the nerve or nerve force; but it may be very much doubted whether we should be justified in calling it an *increase* of its natural *polarized* condition.

Since, then, the nerve fibre and the muscular fibre present this peculiar polarized condition, we may, without any hesitation, infer, that the two forces, the muscular and the nerve force, are both POLAR, and consequently, that muscular action and nerve action are *polar* actions.

During life the muscular and nerve fibre may be considered as existing in a state of *tension*,* a *forced* state, and muscular relaxation may coincide, and would be synonymous with, this polarized condition or state of *tension* and *contraction*, the result of a *depolarization* of the muscular fibre. In the normal state this *depolarization* is induced by means of nervous agency, but it may also occur from other means, such as chemical or mechanical agencies, or whatever is likely to produce disorganization. *Depolarization* having occurred, the polarized state is easily and readily restored by the act of *nutrition*; hence *contraction* may be partly considered as the result of *molecular attraction.*[*] The particles constituting the muscular fibre being in a state of self-repulsion whilst in this polarized condition, would resist the force of molecular attraction unless depolarized; and, according to this view, the results of muscular contraction would necessarily follow in conformity to the law as laid down by Schwann.

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* It would be of some importance to ascertain the state or condition of the prisms in the electric organ of the fish, prior to their discharge of electricity. Theoretically, it might be supposed that all the tissues in the animal body may be considered as being in an electric or polarized state, presenting, however, great differences in regard to each other.

The nerve-fibre existing also in a state of *tension*, whatever influences this state would also produce a *depolarization* of its fibre, the result being manifested by muscular contraction, or pain. As it is only during the period of this *depolarization*, when the *tension* is altered, that contraction occurs, we have some clue, as pointed out by Dr Todd,[†] to the reason why contraction only ensues at the *opening* or *closing* of the circuit, and not *during* the continuance of the passage of the current along the nerve. The constant current does not induce those momentary changes necessary for the production of muscular contraction; it is only at the *opening* and *closing* of the circuit that the *tension* of the fibre is affected. The question, however, may be asked, How do we account for the fact that an *inverse* current through a nerve will cause *tetanic contractions*, whereas a *direct* current has no such effect? In this instance we believe the *inverse* current to produce an *altered* condition of the *polarized* state; but we do not believe, as has been already stated, that it *increases* the normal polarized condition, and if contraction be due to momentary changes in the condition of the nerve, the tetanized state of the muscle might be a necessary consequence until the nerve had acquired its normal and permanent condition. But the same remarks might be made of the nerve in which the *direct* current has passed, and yet no contraction occurs. This is a difficulty which at present we cannot satisfactorily overcome, but the fact is of some importance as indicating a dependence upon the *direction* of the electric current in its passage along the nerve, and showing that a peculiar state is induced in one case and not in the other.* Our endeavours to ascertain what this state may be by means of the galvanometer have hitherto failed.

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[*] We have already alluded in our former paper to the opinion of Dr Radcliffe, who considers that muscular contraction is the result of molecular attraction.

[†] Cyclopædia of Anatomy and Physiology; *Art.* "Physiology of the Nervous System."

In all these discussions the importance of considering the conditions both of the nerve tissue and muscular tissue, in regard to their *nutrition*,† cannot be too strongly insisted upon. It is by nutrition that the *force* is maintained, and it is by this act that it is renewed. If the changes in each tissue are not properly carried on, or should fail in one or both of them, spasms or convulsions may be induced at one time, or the contrary effect, such as paralysis, might occur at another.

It may be urged as an argument against the views now taken of *muscular* force and *nerve* force being *polar*, that they are considered as identical. So far as they are POLAR they are identical; but the conditions under which the force exists in the two tissues differs. In the muscle it is limited to, and associated with, that tissue, and may be considered as existing in a *static* form. In the nerve it may also exist in this form, the *static*, but it is capable also of being *transmitted* from one point to the other, in consequence of the peculiar organization of the nervous tissue. The identity between the two forces removes those difficulties that might otherwise arise in considering their reaction one upon the other. *Muscular force* and *nerve force* must not be supposed to be *produced* by their respective tissues, but *associated* with them; the *modes* under which they are manifested differ, and would indicate that the *form* under which *nerve force* exists would prove it to be of a higher character than that of *muscular force;* and all evidence tends to show that even the *form* under which *muscular force* exists points it out as of a higher character than any of the other *forms* of *polar force* as manifested in the inorganic kingdom.*

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* Matteucci believes that the *excitability* of the nerve is increased by the *inverse* current, and diminished by the *direct* current. Phil. Trans. 1846, part iv., p. 483.

† We cannot do better than refer to the article on the "Physiology of the Nervous System" in the Cyclopædia of Anatomy and Physiology, by Dr Todd, in which this importance is clearly pointed out.

It may be urged, if *nerve force* be a *polar force*, how is it possible to conceive that mental acts, that phenomena connected with the *mind,* should be the result or the manifestation of *polar action*? Without entering into the discussion as to how far mental acts are the *result* of an action of the brain, or whether it be through the *medium* of the brain that the mind acts upon the body, or whether mental phenomena are *polar* phenomena, we nevertheless believe that the phenomena associated and connected with nervous action are confined to, and can only become manifested in the animal kingdom; and although they may bear a close relation or *connection* with the action of other *polar forces,* they will nevertheless form a distinct class, a class *sui generis,* and perhaps the highest *form* by which *polar* phenomena can become manifested.

On the Base of the Carboniferous Deposits, and the Lower "Old Red Sandstone." By W. S. SYMONDS, F.G.S., Rector of Pendoch, Worcestershire.

In my paper "On the Transition Beds from the Upper Silurians into the Old Red Sandstone, and from the Old Red Sandstone into the Carboniferous rocks, in Herefordshire and Gloucestershire,"† I drew the attention of geologists to two sections in the Forest of Dean, where the yellow sandstone, below the lower limestone strata of the Carboniferous limestone, passes *conformably* upwards into that lower limestone shale and downwards into red marls, sandstones, and the so-called "Old Red" conglomerate. I have also given a section of these beds in my work, "Stones of the Valley," taken by my friend M. Phil De la Harpe of Lausanne.

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* *Vide* Faraday's paper on the Gymnotus (Experimental Researches, vol. ii., p. 16), on the "Relation between Nervous Power and Electricity."

† Edinburgh New Philosophical Journal, October 1856, p. 239.

In ranking this yellow sandstone as the upper member of the "Old Red," or rather as a passage group of deposits from the Old Red Sandstone into the Carboniferous deposits, I followed the example of Sir C. Lyell,* Sir R. Murchison, and Hugh Miller;† but having paid some attention to the subdivision of the Carboniferous system in Ireland, and the relation of the Irish *real representative* of our Herefordshire Old Red Sandstone to the inferior and superior rocks, I would express my opinion that the yellow sandstone of *Ireland* is a Carboniferous deposit, separated by an excellent boundary line (a physical *break* below the conglomerate) from the true "Old Red" of that country. The rock which in England we have been in the habit of calling "Old Red" conglomerate, is the first formation of the Carboniferous group of Ireland. There can be no mistake about it.

Above this conglomerate there are deposited a series of red sandstones and shales between 200 and 600 feet thick, which pass into whitish and yellow-coloured sandstone, with *Cyclopteris Hibernicus* and *Anodon Jukesii.*

I cannot doubt the correlation of these rocks in Ireland with our English deposits known as the "Old Red" conglomerate of the Forest of Dean, the Clees, and the Blorenge, and which is also surmounted by red and yellow sandstones.

The fossils of the yellow sandstone are peculiar, and link together, as it were, the group of Carboniferous organisms with those of the true "Old Red." In Ireland, the *Cyclopteris Hibernicus* fern, the fresh-water shell, *Anodon Jukesii*, and an occasional scale of the Holoptychius, are the characteristic fossils; but the Holoptychius is eminently a CARBONIFEROUS fish. It may be interesting to the geologist to be informed that *fertile fronds* of the *C. Hibernicus* have been discovered near Waterford in the yellow sandstones. Of this discovery, Dr Melville, the Sweeney Professor, writes me word that, "in the fertile fronds, the pinnules of the primary pinnæ are divided into capillary segments, subelevate at their extremities; but the intermediate pair of pinnules, the peculiar characteristic of the fern, are uncut, and the terminal pin-

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* Man. El. Geology, 5th Edition, 416. Siluria, p. 254.

† Old Red Sandstone, p. 200, &c.

nules of the primary and secondary rachides, as might be expected, do not exhibit the capillary segmentation."

In Scotland, these yellow sandstones furnish abundant Holoptychii and a Pterichthys, a characteristic organism in Scotland of the *lowest* "Old Red" deposits.

In England, Mr Baxter of Worcester has discovered a Pterichthys in these yellow sandstones, below the Carboniferous limestone of Farlow, N.W. of the Titterstone Clee, in Shropshire, and to which Mr Roberts of Kidderminster has added scales of Holoptychius or Megalicthys, and a fern as yet undescribed. Of the Pterichthys, Sir P. Egerton writes me word that it resembles, "in the large size of the head and shortness of the carapace, *P. hydrophilus* (*Pamphractus hydrophilus* Ag.) from the Dura Den sandstone." The fern, I suspect, will turn out a Coal Measure form.

As I have already stated, in Ireland there is an unconformability between the Carboniferous ("Old Red") conglomerate and the underlying group of deposits. On my return from Ireland I proceeded with my friend Dr Melville to investigate this point among the sections of the west of England; unfortunately, however, a lame leg prevented my proceeding, and I have been *hors de combat* ever since. I did not, however, fail to call the attention of local geologists to this important subject.

Now, below the Carboniferous ("Old Red") conglomerate in Ireland we have an immense thickness of rocks, which, in my humble opinion, are neither more nor less than the equivalents of the rocks of our Breconshire Van district, from the tilestones to the upper cornstones below the conglomerate. I am much indebted to my friend Mr John Kelly of Dublin, and Mr Du Noyer, for much valuable information upon the rocks in the south-west of Ireland; and through their assistance I was enabled to trace, I believe, the history of the Irish beds, and compare that with our own. The "Glengariff grits," and "Dingle beds" are, I imagine, simply local developments of our English *lowest* "Old Red" deposits (tilestones), which pass conformably upwards into the correlative strata of our lower and upper "cornstones," and which are overlaid unconformably by the true base of the Carboniferous deposits, the conglomerate. The Irish "tilestones" pass downwards conformably, as do ours in the Kington district, &c., into Upper Silurian rocks containing Upper Silurian fossils; but in those Upper Silurian strata to the Carboniferous (" Old Red") conglomerate in Ireland, I believe no fossils, save a few uncertain plant remains, have been detected.

The " Devonian system" in Ireland, as in England, has *no base;* and Mr Kelly wishes to classify all the rocks below the conglomerate, at the base of the coal measures, as Upper Silurian, and would limit the Old Red Sandstone to the conglomerate and sandstones (the yellow sandstone inclusive), which are really the base of the Carboniferous deposits.

But we are every day learning the great lesson, that there is no such thing in geology as universal physical discontinuity; and the unconformability of the Irish Lower Carboniferous rocks to those below is probably a limited and local affair, if we may judge from the passage upwards of a Pterichthys into the yellow sandstone of England and Scotland. Nevertheless, for the sake of convenience, let us take the Irish boundary line as a good British demarcation between the Carboniferous and Old Red systems, and sum up the *fossil* evidence.

What, then, have we? In Ireland, England, and Scotland, the conglomerates and sandstones above appear to belong rather to the Carboniferous than the Old Red, but are linked by the Pterichthys, which, like the Trilobite, passes upwards. In England and Scotland the Old Red proper furnishes abundant remains of ganoid fish, crustaceans, and plants; while in England Old Red crustaceans and fish are found in an Upper Silurian " bonebed," as in Devon Upper Silurian shells are found in Old Red strata. It appears, also, from Pander's splendid treatise on Devonian and Silurian fish, that some twenty species, from beds of *Upper Silurian strata,* have been discovered—*Cephalaspides* and other forms.

Under these circumstances, it may possibly strike the geologist that the term "Old Red" is most applicable to that great intermediate series of strata, representing the lapse of an enormous period of time, between the Upper Silurian " bonebed" and the Carboniferous rocks,—inasmuch as the noblest organisms of those rocks were principally discovered and described by a " working man," who has passed to another world, and left that work which, under the title of "*Old Red Sandstone*," is assuredly more entitled to give a name to the series of deposits of which it treats, than are the shells and corals of Devon and the Rhine.

On the Ancient Physical Geography of the South-East of England. By H. C. Sorby, F.R.S., F.G.S.

It is now several years since the observations described in this paper were made. I intended them to form, eventually, a portion of a far more general treatise on the physical geography of the various periods to which they belong, and had no idea of publishing them in their present incomplete state. So much attention having, however, been directed to the ancient physical geography of the district by Mr Godwin Austen's paper on the Possible Extension of the Coal Measures beneath the South-East of England;* and some of my observations having a strong bearing on the important practical questions raised by him, and also by Mr Prestwich, in his *Geological Inquiry* respecting the water-bearing strata of the country around London, and in his paper on the Boring through the Chalk at Kentish Town,† I have resolved to publish them in their present condition. In so doing, I shall confine myself to such facts as indicate the existence of a land surface or sea during the Permian, Oolitic, Wealden, and Lower Cretaceous periods, over which the deposits of those epochs could or could not be formed, and where the still older Carboniferous strata might perhaps be reached, without having to pass through the whole thickness of those more recent rocks. The method of investigation I shall employ is the same as that made use of in my former paper on "Current-Structures and Ancient Physical Geography," published in this journal,‡ of which this must be considered a continuation and extension—viz., determining the direction of the current from the various structures produced by currents, and adopting such a probable suppositional distribution of land and water as would

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[*] Quart. Jour. of Geolog. Soc., vol. xii., p. 38.

[†] Ibid., p. 6.

[‡] New Series, vols. iii., p. 112; iv., p. 317; v., p. 275.

give rise to similar currents in seas of the modern period. It would be to little purpose to enter into a detailed account of the peculiarities of the currents in the various localities, without the aid of several maps, and without altering the character of this communication, and making it far too long; and therefore I shall confine myself to such general conclusions as bear on the question before us.

My examination of the magnesian limestone has chiefly been confined to the south of Yorkshire, where I have very thoroughly explored it in minute detail over a length of twenty-three miles. The currents present appear to be divisible into two groups; one due to the rise and fall of the tide, and the other produced by the action of waves stranding on shoals. At all events, their directions and mutual relationships at the various localities can be easily explained on this supposition. The mean directions of tidal oscillation, derived from the means at nearly a hundred localities, are S. 70°, 30′ W. from 1150 observations, and N. 70°, 25′ E. from 1000 observations. This excess of drifting force from the W.S.W. side, as indicated by the greater number of cases met with, is distinctly shown at nearly every separate locality, and may be explained by supposing that the tidal wave advanced from that quarter. The character of the shoals, where the deposits were influenced by the action of the stranding surface-waves, was apparently somewhat similar to that of those occurring in such a shallow tidal sea as lies along the coast of Belgium.* There were spaces several miles in width without any shoals, and then patches of long narrow banks, elongated in the line of the tidal currents, and separated by tidal channels of deeper water. The mean directions of the currents due to the stranding of the surface-waves on these shoals, are S. 46° E. from 287 observations, and N. 53½° W. from 219 observations, as derived from the means at the various localities, at nearly all of which an excess of drifting force from the S.E. is sufficiently well marked; therefore indicating that the larger waves advanced chiefly from the S.E. Now, as I have argued in my former papers, provided the same amount of effective waves had advanced from all quarters of the compass, and stranded on these shoals, the line of the stranding-wave oscillating currents should have been perpendicular to those of the rise and fall of the tide. However, it differs from it by 28°, and is inclined towards the east side, as though the chief large waves and the principal storms were those due to the action of the east wind. Assuming that the amount of the wind from different points of the compass at the Permian period was similar to what it is now, the actual force and total amount of that from the W. and S.W. would be very much greater than that from the E. and S.E.; and, therefore, provided the distance to the coasts was the same, the waves that advanced from the S.W. would have been far greater and more effective than those from the E. This, however, does not agree with the observed facts, and therefore I think we must conclude that there was some land to the S., whilst the sea was open to the E.; so that the less powerful east winds, blowing over a greater expanse of water, could raise up larger waves and produce rougher seas than the more violent and continuous westerly gales, which had only blown across a much less surface of water. When making a map to illustrate the physical geography of the Permian period, described at the meeting of the British Association at Glasgow in 1855, these considerations led me not to hesitate to put a mass of land in the south of England, stretching across into the continent of Europe. My meaning, and the nature of the argument, will perhaps be more clearly understood by supposing any one to have been placed in the midst of the Permian sea in South Yorkshire, and to have observed that, though the wind from the E. was on the whole less violent than from the W. or S.W., yet that the roughest seas were those produced by the east wind; would he not have been perfectly justified in concluding that there was some land to the S. that protected the sea on that side?

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* See the Chart of England and Holland, by Norrie, and Sailing Directions for the North Sea.

My observations in the Oolitic strata have been chiefly confined to the coast of Yorkshire, and the districts of Bath and Oxford. The facts to be observed in these localities are sufficiently distinct, but still they are such that I scarcely like to attempt to offer any very decided explanation of them, until

I am better acquainted with the intermediate districts. In the neigbourhood of Bath, the general line of the axis of the currents appears to have been on the whole nearly N. and S. At the period of the Inferior Oolite, it was nearly from N.E. to S.W., becoming afterwards more N. and S.; and at the epoch of the Coralline Oolite was about from N.N.W. to S.S.E., as if there had been some gradual but considerable change in physical geography during the Oolitic period. This is not only indicated by the change in the direction of the currents, but also by a variation in their character. During the greater part of the period, there seems to have been little or no tide present, or at least the effect of the tidal currents was extremely small. The currents appear to have been not oscillating, but simple, like those due to the action of prevailing winds, to which I have referred in my paper on the "Old Red Sandstone Sea,"* or to a general outflow of water, like that in the Cattegat, or at the entrance into the Baltic.† During the deposition of the Forrest Marble, however, oscillating currents were present, running in the line of the N. and S. axis; as if, at that period, tidal currents exercised a considerable influence on the deposits.

The only conclusion that can be safely drawn from these data is, that there was some coast-line which caused the currents to take a N.N.E. and S.S.W. direction, and was also so placed in relation to other coast-lines, that the tidal currents were usually very small, or altogether absent. The trend of the older rocks in Wales is such, that a coast-line on that side may have been the chief agent in determining the *direction* of the currents; but it appears difficult to understand why the tidal action was so very slight, unless there was also some land to the E. If so, the absence of tidal currents might have been occasioned either by there having been a confined opening to the ocean, or by there having been two openings, so placed that the two tidal waves interfered, in the same manner as now takes place in the Irish Sea, where the wave passing round the south coast of Ireland meets and interferes with that passing round the north coast. On this account, over a space of about ten miles in diameter, half way between the Calf of Man and Ireland, is a still place, characterized by a bottom of blue mud, though the rise and fall of the tide is from sixteen to twenty feet; whilst to the N. and S. the currents gradually increase up to three or four miles per hour.* This supposition would agree with the presence of a sufficiently decided tidal action during the deposition of the Oolitic strata on the coast of Yorkshire; for it would involve only a local, and not a general absence of tidal currents. Still, I must admit, that the facts are not so numerous or so definite as could be wished. So far as they go, they certainly agree very well with the supposition of dry land having existed in the east of England during the Oolite period; but it cannot be said that they *prove* its existence.

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* Edinburgh Philosophical Journal, New Series, vol. iii., p. 112.

† Sailing Directions for the Cattegat, the Sounds, and the Belts, by Laurie; and for the Cattegat and Baltic, by Norrie.

The sandy beds of the Wealden deposits are often very full of excellent current-structures. In very many cases they prove that oscillating currents were present; which agree so well with what would be produced by the rise and fall of the tide amongst a number of irregular banks at the mouth of a large river, that, considering all the circumstances of the case, it appears to me to be the most probable explanation. The deposits of clay have been formed in more tranquil water, and may perhaps indicate more lacustrine conditions or periods when the tide was barred out. My observations commenced in the neighbourhood of Tunbridge Wells, and were much impeded by the absence of sufficiently numerous good sections. What I take to have been the currents due to tidal oscillation give means of N. 67° W. from sixty-seven observations, and S. 63° E. from fifty-nine observations; there being thus a slight excess from the west. This may be explained by supposing either that the tide advanced from the west, and there was scarcely any stream from the river, or, probably still better, by supposing that the river ran from the west with a sufficiently strong stream to counteract the excess of action in the line of the advancing tidal wave. These currents appear to have run amongst a number of shoals on which surface-waves stranded, so as to give rise to oscillating currents yielding general means of S. 67° W. from sixty-eight observations, and N. 68° E. from thirty-four observations. This considerable excess of action from the west side agrees with the supposition that the most effective waves were produced by western or south-western winds. There are also some dubious currents from the south-west, with little or no oscillation, which may have been due to the river, or to the drifting action of the south-west wind. Though these conclusions are what appear to me to be the most probable, yet I must confess that the facts are not so decided and distinct as to prove them beyond the possibility of doubt, but still are sufficient to justify us in adopting them provisionally.

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* Beechy's paper on the "Tide in the Irish and English Channel," Phil. Trans., 1848.

The Hastings sand, near Hastings, indicates a less amount of shoals than at Tunbridge Wells. The general means of what I take to have been tidal oscillation are S. 81° E. from eighty-nine observations, and N. 79° W. from sixty-two observations. These differ from those at Tunbridge Wells in showing an excess from the E. which, however, may be explained, by supposing that at Hastings the action of the river was less, and that of the tide greater, which supposition would also agree with the less amount of the shoals. The mean directions of the stranding wave oscillations are N. 21½° E. from twenty-one observations, and S. 38° W. from nine observations; thus indicating that the excess of waves came from the N. or N.E. side. There were also some distinct currents without any oscillation, chiefly running from the west side, giving a mean direction of N. 68° W. from fifty-five observations, or nearly in the same line as the tidal axis. Perhaps these were due to the action of the river in places and at periods when the influence of the tide was only very limited.

Taking, then, all the above facts into consideration, the most probable conclusion appears to be, that the line of the axis of the Wealden estuary, in the eastern part of Sussex, was from W.N.W. to E.S.E.; that the river ran from the W. side; and that it opened into a tidal sea towards the E. This would therefore lead to the inference that there was a boundary of land towards the N. Since there was an excess of stranding-wave action from the north side at Hastings, but from the south side at Tunbridge Wells, provided that the prevailing winds of the period were, as is the case now, from the S.W. or W.S.W., it appears to be the most probable that the deposits at Hastings were formed in a part of the estuary that was considerably nearer to the south coast than to the north; for if not, the stronger and more prevailing S.W. winds would have caused the excess to have been from that side. This does happen at Tunbridge Wells; and the only conclusion that can be drawn from it is, that the coast-line to the N. was not so much farther than that to the S. as to cause the greater waves to have advanced from that side. These data for estimating the distance of the north coast may be conveniently symbolized by making $n =$ the distance of the north coast at Tunbridge Wells, and $s =$ the distance of the south coast; and taking the distance between the axial parallels of Tunbridge Wells and Hastings at 12 miles, we thus have, from the above data,

$n + 12$ considerably greater than $s - 12$,
n not much greater than s;

Therefore,

n considerably greater than $s - 24$, but not than s.

Now, since the extension of the Wealden deposits indicates that the south coast could not have been less than 12 miles S. of Hastings, we must have $s =$ fully 24; and in this case we must have $n =$ considerably more than 0, but not than 24 miles. This would place the north boundary of the estuary somewhere S. of the line of the modern Thames. If the south coast was 25 miles S. of Hastings, the north coast was probably nearly in the position of the Thames.

I have made many very careful observations of the directions of the current-structures in the greensand of the neighbourhood of Folkestone, in a tract extending 7 miles from E. to W. and 2½ from N. to S., having Folkestone at the S.E. and Stanford at the N.W. corner. In the upper part of the Lower Greensand there is much drift-bedding, and many good sections are exposed, so that I could determine the direction of the currents with great accuracy in about a dozen localities. In none of these was there any indication of an oscillating current; it appears to have been always from one side, without any great amount of variation. The direction was very uniform throughout the whole tract, the extreme difference in the means at the various localities being only 32°. The mean of all the local means is N. 25° W. from 120 observations. There appears also to have been no considerable variation in the direction of the currents between the periods of the upper and lower part of the upper portion of the Lower Greensand, since in one very good section the mean directions only differed by 1°. I saw no decided current-structures in the lower portion, as though the currents were either very gentle or did not extend to the bottom on account of the greater depth. The direction of the currents at the period of the Upper Greensand did not differ sensibly from that of those present during the deposition of the Lower, since, at the only locality where I could find a good section, the mean was N. 16° W.

It is therefore sufficiently well proved, that in the neighbourhood of Folkestone there was a simple, general current from about N.N.W., in a sea so little influenced by tidal currents that no decided evidence of their presence can be recognised. So far as I have hitherto been able to determine, this general northerly current prevailed past Maidstone to Sevenoaks; but my observations there are only very limited and imperfect. In the Isle of Wight the current-structures are extremely well developed in the Lower Greensand, and may be seen to great advantage in the coast-sections. They however differ from those near Folkestone in indicating an oscillating current, as if due to the rise and fall of the tide along nearly the same general N.N.W. and S.S.E. line as the simple currents at Folkestone. Probably, therefore, the absence of oscillating currents at that locality was occasioned, not by any want of a connection with a tidal sea, but by some such distribution of land and sea as caused the tidal currents to be so small as to be entirely counteracted by a general, regular, simple current.

It will thus be seen that during the passage from the Wealden to the Lower Greensand period, the line of the currents changed from W.N.W. to N.N.W., as though the subsidence that must have occurred so as to change fluviatile or fluviomarine into purely marine conditions, had given rise to a far greater extension of water towards the N.N.W. Judging from the very common connection between the direction of modern marine currents and the contiguous line of coast, it appears in the highest degree probable that some coast-line extended in a N.N.W. and S.S.E. direction, possibly at no great distance from the S.E. of England. It is impossible, in the present state of the inquiry, to say how far N. it extended in that direction, or whether it afterwards turned somewhat towards the W.; but still the above facts seem to me to point very strongly to the conclusion, that the whole district of the modern Thames was Lower Greensand sea. I think that so very regular and general a current from the N.N.W. could scarcely have existed near Folkestone, unless there was an open sea in that direction to a greater distance than the estuary of the Thames.

The general conclusions to which the above facts appear to lead are, that during the Permian and Oolitic periods land most probably existed in the S.E. of England, the exact position of which still remains to be determined; at the Wealden period the presence of land in the district of the lower part of the Thames is almost certain; but at the epoch of the Lower Greensand this had been in great measure submerged and did not extend so much to the W. If these deductions be compared with the results at which Mr Godwin Austen has arrived, from an entirely different class of facts and train of argument, it will be seen that there is a remarkably close correspondence in many particulars. Indeed, the only decided difference is in respect to the Lower Cretaceous period. For reasons already given, I cannot but conclude that the Lower Greensand sea extended considerably farther to the N.N.W. of Folkestone than indicated by his map.* I fully agree with him in considering it in the highest degree probable that there was land at no great distance to the N.E. of Folkestone; and the prolongation northwards of the axis of Artois would correspond extremely well, both in position and direction, with what would readily explain the facts I have described; but though land *may* have existed to the N.N.W. of Folkestone, the facts are strongly opposed to the supposition of it having been very near. If, then, my deductions are correct, it may be considered extremely probable that Permian, Oolitic, and Wealden strata are absent below the chalk in the district surrounding London, whereas Lower Greensand may be expected to occur. In the boring at Kentish Town, described by Mr Prestwich,* instead of Lower Greensand of the ordinary character being found, a series of sandstones and clays were met with which had mineral characters closely resembling those of the New Red Marls; yet the few fossils that were obtained were Cretaceous species, the only doubt being whether they had fallen down the bore-hole from the undoubted Cretaceous strata passed through at a less depth. The evidence, therefore, does not satisfactorily prove whether they are a local variety of Lower Greensand or New Red Marls. The facts I have described in this paper admit of a far more simple explanation, if we conclude that they are Lower Greensand. Not only does the actual direction of the currents at Folkestone point to the probable extension of the Greensand sea over the London district, but the great similarity in the direction during the periods of both Upper and Lower Greensand indicates that there was no considerable change in physical geography. The mean of all the means in the Lower Greensand was N. 25° W., and, at the single locality where I was able to examine a section, the mean direction in the Upper was N. 16° W., which does not differ from it more than the variation at different localities. Now, the existence of Upper Greensand at Kentish Town is fully established by the boring; and, unless that locality was situated very close to the limit of the land at the period of the Lower Greensand, and also very close to the limit of the sea at that of the Upper, so that the change from land to sea produced only a very small alteration in the position of the coast, it seems difficult to understand why the direction of the currents should have remained nearly the same. The marly and clay-like character of the Upper Greensand is, however, strongly opposed to that supposition; and therefore, though I admit that the facts are not sufficient to *prove* it, yet they strongly point to the conclusion, that, like the Upper, the Lower Greensand does also extend over the London district, and that the land-surface lay more to the E. I should, however, be sorry to press this argument strongly, for if a greater number of sections of the Upper Greensand could be seen near Folkestone, the general mean direction might be considerably modified. An examination of the Greensand in the neighbourhood of Cambridge would, in all probability, throw much light on the question; since, if a general current from the N.N.W. was also present in that locality, the continuity of the sea over the intervening district might be considered to be in the highest degree probable. The important practical bearing of this question, in connection with the supply of water or of coal for London, will be seen on consulting Mr Prestwich's and Mr Godwin Austen's papers already cited.

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* Quarterly Journal Geological Society, vol. xii., p. 46.

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* Quarterly Journal Geological Society, vol. xii., p. 6.

I am most willing to admit that the above conclusions may require very considerable modification when a greater number of facts are known, My object has been to describe such as I have been able to collect, so as to make them available in connection with the various questions to which they relate, and to draw from them as simple and straight-forward conclusions as they admit of. Farther research may show that some exceptional peculiarities have exercised most important influence in the localities I have explored; but of course this can only be decided by a very extensive series of observations. However, it appears to me that, in the present state of the subject, it is the safest and most legitimate method of inquiry to adopt such explanations as are the simplest and most in accordance with the common and general peculiarities of the currents in modern seas, and to leave all the smaller details and exceptional cases to be cleared up by subsequent observations. Though, by thus attempting to draw definite conclusions from confessedly imperfect data, I run the risk of committing serious errors, yet it seems to me better to do this than to leave the subject untouched, or to describe the facts without attempting to draw any conclusions. One person alone cannot do full justice to such an extensive subject; but by thus pointing out the kind of results that may be arrived at, I hope to induce others to enter on a method of research which I am convinced will ultimately lead to many valuable results.

Theory of Linear Vibration—(continued). By Edward Sang, Esq., F.R.S.E.

II.—*General Method of effecting the Integration—Resulting Law.*

If there be a number of bodies, A, B, C, . . K, L, M, moving in space, and attracting each other with intensities proportional to their mutual distances, and to some co-efficient peculiar to each pair; we may represent the attraction subsisting between two of them, as A and B, by the symbol $\overline{\alpha\beta}$. AB, in which AB is the linear distance between the two bodies, and $\overline{\alpha\beta}$ is a coefficient depending on the relation of A to B. Similarly, $\overline{\alpha\gamma}$. AC would stand for the atttraction subsisting between A and C; and so on. The symbols $\overline{\alpha\beta}$, $\overline{\alpha\gamma}$, $\overline{\beta\gamma}$, &c., thus represent constant quantities, depending on the conditions of the system; when one body A is only connected with some of the other bodies, the values of the coefficients for those bodies which do not act on it, become zero. Thus $\overline{\alpha\delta} = 0$ would imply that there is no attraction or *liaison* between A and D.

Since the cosine of the inclination of the line A B to the direction x, is

$\frac{x_B - x_A}{AB}$, the attraction $\overline{\alpha\beta}$. AB estimated in the direction x

is $\overline{\alpha\beta}\,(x_B - x_A)$, wherefore the whole tendency to cause the body A to move in the direction x is $\overline{\alpha\beta}(x_B - x_A) + \overline{\alpha\gamma}(x_C - x_A) + \overline{\alpha\delta}(x_D - x_A)$ + &c., and similarly for the other bodies of the system; so that the second derivatives of the positions of the various bodies are given by the following equations:—

$$\left.\begin{array}{l}
A\,.\,{}^{2t}x_A=\overline{\alpha\beta}\,(x_B-x_A)+\overline{\alpha\gamma}\,(x_C-x_A)+\overline{\alpha\delta}\,(x_D-x_A)+\&c.\\
B\,.\,{}_{2t}x_B=\overline{\alpha\beta}\,(x_B-x_B)+\overline{\beta\gamma}\,(x_C-x_B)+\overline{\beta\delta}\,(x_D-x_B)+\&c.\\
C\,.\,{}_{2t}x_C=\overline{\alpha\gamma}\,(x_A-x_C)+\overline{\beta\gamma}\,(x_B-x_C)+\overline{\delta\gamma}\,(x_D-x_C)+\&c.\\
D\,.\,{}_{2t}x_D=\overline{\alpha\delta}\,(x_A-x_D)+\overline{\beta\delta}\,(x_B-x_D)+\overline{\gamma\delta}\,(x_C-x_D)+\&c.\\
\quad\&c. \qquad\qquad \&c. \qquad\qquad\qquad \&c. \qquad\qquad \&c.
\end{array}\right\}\quad(13.)$$

In order to prepare these equations for being integrated, I multiply them respectively by the arbitrary quantities a, b, c, d, &c., and take the sum of all the results. This operation gives

$$aA\,.\,{}_{2t}x_A+bB\,.\,{}_{2t}x_B+cC\,.\,{}_{2t}x_C+dD\,.\,{}_{2t}x_D+\&c.=$$

$$\left.\begin{array}{l}
x_A\{\overline{\alpha\beta}\,(b-a)+\overline{\alpha\gamma}\,(c-a)+\overline{\alpha\delta}\,(d-a)+\&c.\}\\
+x_B\{\overline{\alpha\beta}\,(a-b)+\overline{\beta\gamma}\,(c-b)+\overline{\beta\delta}\,(d-b)+\&c.\}\\
+x_C\{\overline{\alpha\gamma}\,(a-c)+\overline{\beta\gamma}\,(b-c)+\overline{\gamma\delta}\,(d-c)+\&c.\}\\
+x_D\{\overline{\alpha\delta}\,(a-d)+\overline{\beta\delta}\,(b-d)+\overline{\gamma\delta}\,(c-d)+\&c.\}+\&c.
\end{array}\right\}\quad(14.)$$

and I then seek to determine the values of the arbitrary multipliers, a, b, c, d, &c., so that the coefficients of the several derivatives on the one side may be proportional to the coefficients of their primitives on the opposite side of the equation; that is to say, if R represent some unknown ratio, we seek to give such values to a, b, c, d, &c., as may satisfy the equations,

$$\left.\begin{array}{l}
aAR=\overline{\alpha\beta}\,(b-a)+\overline{\alpha\gamma}\,(c-a)+\overline{\alpha\delta}(d-a)+\&c.\\
bBR=\overline{\alpha\beta}\,(a-b)+\overline{\beta\gamma}\,(c-b)+\overline{\beta\delta}(d-b)+\&c.\\
\quad\&c. \qquad\qquad \&c. \qquad\qquad \&c. \qquad\qquad \&c.
\end{array}\right\}\quad(15.)$$

Since the quantities on the right hand necessarily amount to zero, we have

$$aA+bB+cC+dD+\&c.=0 \quad . \quad . \quad (16.)$$

Now putting ΣA for the sum of all the masses, we have

$$aA+aB+aC+aD+\&c.=a\Sigma A \quad . \quad . \quad (17.)$$

whence, subtracting (16) from (17),

$$(a-b)\,B+(a-c)\,C+(a-d)\,D+\&c.=a\Sigma A \quad (18.)$$

Multiplying this equation (18) by AR, and putting, for shortness

$$\frac{\mathrm{R}}{\Sigma\mathrm{A}}=\mathrm{S} \quad . \quad . \quad . \qquad (20.)$$

we obtain

$$a\mathrm{AR}=\mathrm{ABS}\ (a-b)+\mathrm{ACS}\ (a-c)+\&\mathrm{c.} \quad . \qquad (21.)$$

and subtracting this from the first of the equations (15), as also operating similarly on the others, we find

$$\left.\begin{array}{l} 0=(\mathrm{ABS}+\overline{\alpha\beta})\ (b-a)+(\mathrm{ACS}+\overline{\alpha\gamma})\ (c-a)+\&\mathrm{c.} \\ 0=(\mathrm{ABS}+\overline{\alpha\beta})\ (a-b)+(\mathrm{BCS}+\overline{\beta\gamma})\ (c-b)+\&\mathrm{c.} \\ 0=(\mathrm{ACS}+\overline{\alpha\gamma})\ (a-c)+(\mathrm{BCS}+\overline{\beta\gamma})\ (b-c)+\&\mathrm{c.} \\ \quad\&\mathrm{c.} \qquad\qquad \&\mathrm{c.} \qquad\qquad\qquad \&\mathrm{c.} \end{array}\right\} \quad (22.)$$

Now we observe of these equations (22), that the sum of the right hand members is zero, so that, if n be the number of bodies, there are only $n-1$ of these equations effective. And again, any multiplication of all the coefficients, a, b, c, &c., by a fixed number, can produce no essential change; so that one of these coefficients may be arbitrarily assumed without detriment to the generality of the investigations; wherefore of the differences $(b-a)$, &c., we have to determine $n-2$; and thus the $(n-1)$ effective equations (22) are sufficient to determine these and the unknown ratio R, or its equivalent S.

If, in order to obtain S, we eliminate the differences $(a-b)$, $(b-c)$, $(c-d)$, &c., from the $n-1$ equations (22), we must necessarily obtain an equation in which S rises to the $n-1^{\mathrm{st}}$ power, of which the general form is

$$\ldots \mathrm{S}^{n-1}+\ldots \mathrm{S}^{n-2}+\ldots \mathrm{S}^{n-3}+\ \&\mathrm{c.}=0 \quad . \qquad (23.)$$

and thus it seems that, if all the roots of this equation be real, S has $n-1$ distinct values. It is not difficult to show that for the coefficients of elasticity $\overline{\alpha\beta}$, $\overline{\beta\gamma}$, &c., positive, the values of S must be all negative; but it is not quite so easy to show that all the roots of the equation are real. Without going into that matter, which would indeed lead us too far from the real subject, viz., *linear* vibration, I remark, that for each root of the equation there must be a distinct set of values of the multipliers, a, b, c, &c.; so that if S_1, S_2, S_3, &c., represent the several roots of equation (23), a_1, a_2, a_3, &c.,

b_1, b_2, b_3, &c., and so on, may represent the corresponding values of the several multipliers; also for each value of S we shall have a value of R.

Let, then, ν be any integer number less than n, and

$$R_\nu = S_\nu \, \Sigma A \quad . \quad . \quad . \qquad (24.)$$

will represent one of the values of R; so that equation (14) becomes

$$\left.\begin{array}{l} a_\nu A \, . \, {}_{2t}x_A + b_\nu B \, . \, {}_{2t}x_B + c_\nu C \, . \, {}_{2t}x_C + \&c. \\ = R_\nu \, \{a_\nu A \, . \, x_A + b_\nu B \, . \, x_B + c_\nu C \, . \, x_C + \&c.\} \end{array}\right\} \qquad (25.)$$

or if, for the sake of shortness, we put

$$a_\nu A \, . \, x_A + b_\nu B \, . \, x_B + c_\nu C \, . \, x_C + \&c. = X_\nu \qquad (26.)$$

$${}_{2t}X_\nu = R_\nu \, . \, X_\nu .$$

Whence, for positive values of R we have

$$X_\nu = U_\nu (e^{+t\sqrt{R_\nu}} + (e^{-t\sqrt{R_\nu}}) + V_\nu (e^{+t\sqrt{R_\nu}} - e^{-t\sqrt{R_\nu}}) \qquad (27.)$$

an equation which falls to be used in the case of repulsion, and in which U and V are quantities determined by the condition of the system at a given epoch; to this equation we need not further advert. Also, for negative values of R

$$X_\nu = U_\nu \sin \left(t\sqrt{-R_\nu} + u_\nu\right) \quad . \quad . \qquad (28.)$$

in which U_ν and u_ν are constants introduced during the integrations, the first having reference to the extent, the second to the epoch of an oscillation.

Exactly similar reasoning may be followed in regard to the other co-ordinates y and z, and it is evident, from the nature of the operations, that identically the same equations (15) would be obtained; so that the values of R; a, b, c, &c., which relate to the ordinates x, also relate to the ordinates y and z. Therefore, the complete solution of the problem before us is contained in the following equations:—

$$\left.\begin{array}{l} a_\nu A \, . \, x_A + b_\nu B \, . \, x_B + c_\nu C \, . \, x_C + \&c. = X_\nu \\ a_\nu A \, . \, y_A + b_\nu B \, . \, y_B + c_\nu C \, . \, y_C + \&c. = Y_\nu \\ a_\nu A \, . \, z_A + b_\nu B \, . \, z_B + c_\nu C \, . \, z_C + \&c. = Z_\nu \end{array}\right\} \qquad (29.)$$

$$\left.\begin{aligned} X_\nu &= U_\nu \sin\,(t\sqrt{-R_\nu}+u_\nu) \\ Y_\nu &= V_\nu \sin\,(t\sqrt{-R_\nu}+v_\nu) \\ Z_\nu &= W_\nu \sin\,(t\sqrt{-R_\nu}+w_\nu) \end{aligned}\right\} \quad . \qquad (30.)$$

If we suppose X_ν, Y_ν, Z_ν, to be the co-ordinates of an imaginary point ν, which, for the sake of facility of expression, I shall call the *nucleus of oscillation*, this point ν must describe an ellipse, the centre of which coincides with the centre of gravity of the system, and the position and magnitude of which are determined by the values of U_ν, V_ν, W_ν; u_ν, v_ν, w_ν. The periodic time of the revolution of the point ν in its orbit must be

$$T_\nu = 2\,\pi(-R_\nu)^{-\frac{1}{2}}$$

and neither the periodic time nor the orbit is changed by the mutual attractions of the bodies.

But for each one of the roots of equation (23) we have a distinct set of values, so that for each of the $n-1$ values of ν we have a distinct nucleus of oscillation: and thus it appears that, in such a system of attracting bodies, there are $n-1$ points each of which describes its own ellipse around the centre of gravity in its own periodic time. These nuclei may be regarded as pseudo-centres of gravity; that is, as the centres of gravity, not of the masses A, B, C, &c., but of fictitious masses $a_\nu A$, $b_\nu B$, $c_\nu C$, &c.; and the centre of gravity itself may be classed amongst these, and may be regarded as that ν which results from supposing the multipliers a, b, c, &c., all equal to each other, or their differences all zeroes. The R corresponding to the centre of gravity is zero; that is to say, the periodic time of the centre of gravity is infinite; in other words, its motion, if it have any, must be rectilineal.

Reckoning the centre of gravity amongst them, there are as many nuclei whose positions can be computed as there are moving bodies; and, therefore, by help of equations (29), which are linear, the position of each one of the bodies may be ascertained.

Now it is obvious that the elimination of the quantities x_A, x_B, &c., from the first set of equations (29), must be exactly similar to the elimination of the y's from the second set, or of the z's from the third set, and therefore the compositions of the values of the ordinates x, must be exactly similar to those of the ordinates y. These values, then, will take the forms

$$\left.\begin{aligned} x_A &= (1_A)\,X_1 + (2_A)\,X_2 + (3_A)\,X_3 + \&c. \\ y_A &= (1_A)\,Y_1 + (2_A)\,Y_2 + (3_A)\,Y_3 + \&c. \\ z_A &= (1_A)\,Z_1 + (2_A)\,Z_2 + (3_A)\,Z_3 + \&c. \end{aligned}\right\} \quad (31.)$$

and similarly for the other bodies of the system.

From this it follows, that the motion of each one of the attracting bodies may be regarded as composed of $n-1$ elliptic motions superadded to each other; and that, therefore, the path of each must be a complex epicycloid traced by carrying the centre of one ellipse round the circumference of another, the centre of that other round the circumference of a third, and so on.

The values of the quantities U, V, W, which determine the magnitudes of the oscillations of the nuclei, and the values of u, v, w, which determine their epochs, depend on the accidental conditions of the system; that is, on the impulses which had been communicated to the various bodies before they were abandoned to the effects of their mutual attractions; the coefficients (1_A), (2_A), (1_B), (2_B), &c., which regulate the distribution of the motions of the nuclei among the bodies, depend, on the other hand, on the masses of the bodies and on the coefficients of attraction.

Among the infinity of original impulses which we may imagine to have been given, there may be some that render one or more of the orbits zero, or that may render them all zeroes but one. In such a case it follows that the bodies A, B, C, &c., would describe similar ellipses around the centre of gravity, and that the epochs of their anomalies would be the same; the orbits, too, would all lie in one plane. In such a case the inhabitants of the several planets would perceive no angular change of position in the bodies composing their system; they would simply appear to approach and recede.

But if two of these elliptic motions were to coexist, the phases would be different, both because their periodic times would be unlike, and because they would be differently distributed among the several planets.

The motions of a system of bodies attracting each other in this peculiar way have little interest for any but the speculative analyst. Such arrangements can scarcely occur in nature. The investigation which I have given includes, however, that of the motions of a linear elastic series, and, on that account, becomes of great practical importance.

I may be allowed to remark that this is the first case of the PROBLEM OF THREE BODIES which has been resolved when the resultants of the attractions do not all pass through one point; and I may be excused in adding, that however lamely the investigation may have been given, its intrinsic importance, and the interest which attaches to it, as almost the first pioneer on the road to the great problem of physical astronomy, can well assert its claim to the close attention of every student of physico-mathematics.

The resolution of the analogous problem, when the attractions vary as the second inverse powers of the distances, is of the highest importance in physical astronomy. This importance has procured for it the most intense exertions of the ablest analysts; yet not a single step has been made toward its resolution, and physical astronomy remains a collection of approximative methods, of great value indeed, but ill fitted to satisfy the mind.

The difficulties which surround the subject are enough to dishearten the most sanguine; nor is this success in another variety of the general problem calculated to raise a more than ephemeral hope; because in this particular variety of the law of attraction the variations of the three co-ordinates are already separate, whereas in the actual law of planetary attraction they are intimately involved.

The peculiar oscillations of the bodies A, B, C, &c., may be regarded as modifications of a set of phenomena of which I gave the analysis in the *Edinburgh Philosophical Journal* for April 1832.

When a long wire, fixed at one end, and carrying a polished ball on the other, is bent, and allowed to vibrate, the image of a light seen in the polished surface reveals the path of the vibrating extremity. This phenomenon was first pointed out by Professor Wheatstone. On analyzing the conditions of the motion I discovered that every wire, whatever may be the form of its cross section, has its directions of greatest and least rigidity at right angles to each other, and that the vibrations in these two directions go on simultaneously, but without interfering with each other. Mr Wheatstone had used the ordinary round wire, of which the coefficients of rigidity are nearly equal, and the curves which he obtained were ellipses and elliptic epicycloids ; but my analysis showed that a change in the form of the wire might produce any one of the classes of curves represented by the equations

$$x = \mathrm{U} \sin(\theta t + u)\, ;\ y = \mathrm{V} \sin(\varphi t + v),$$

and that, if the wire be struck so as to cause it to vibrate in parts, other terms analogous to these come to be superadded.

When the wire is bent, motions in the direction z are introduced, and the complexity of these motions affords, perhaps, the best and readiest illustration of the movements of our imaginary system of attracting planets.

III. *Irregular Linear Series.*

When a system of bodies, A, B, C, &c., connected by elastic ties, is distributed in space, the investigation of their motions is attended with great difficulty, because the intensities of their attractions are proportional, not to their mutual distances, but to the variations in their distances; and thus the motions in the directions x, y, and z, are intricately mixed. But when the bodies are all ranged in one straight line, to which their motions are confined, the investigation can be completed by an operation analogous to that which I have just explained.

Such a series may be represented by a number of balls ranged in a straight line, and linked together by means of helical springs. In the general statement of the problem the bodies must be supposed to be of various weight, and the springs to be of various degrees of stiffness. Complete generality would also require that one of the bodies, as E, should not merely be connected with its antecedent D and consequent F, but that it should be connected also with every other body in the series. The method which is given in the preceding part applies directly to this general supposition, and indeed, as will become manifest immediately, no other change is needed in the formula than to suppose that the ordinates x of the various bodies, are to be reckoned, not from the common centre of gravity, but from the mean position of each body. It is more interesting, and is also quite sufficient for my present purpose, to suppose that each body is connected only with those adjoining to it.

I shall, throughout this inquiry, assume as the linear unit twice the distance through which a heavy body falls in the first unit of time. The mass of a body I shall consider as represented by its weight, and I shall define the coefficient of elasticity of a spring (or its stiffness) to be the weight which would be necessary to distend or to compress it through one linear unit. By this arrangement the formulæ are rendered applicable to all units of time, and to all intensities of gravitation.

Let then x_A, x_B, x_C, &c, be the ordinates of the different bodies, each measured from the place which the body would occupy, if all the springs were uncompressed, so that $x_A - x_B$ may be the measure of the compression or distention of the spring placed between them; and let $\overline{\alpha\beta}$, $\overline{\beta\gamma}$, &c, be the coefficients of the elasticity of the different springs; then $\overline{\alpha\beta}\ (x_A - x_B)$ represents the pressure which the spring exerts on each of the two bodies.

The general equations of motion take the form

$$\text{A} \,.\; {}_{2t}x_A = \overline{\alpha\alpha}\,(x_A - x_A) + \overline{\alpha\beta}\,(x_B - x_A)$$

$$\text{B} \,.\; {}_{2t}x_B = \overline{\alpha\beta}\,(x_A - x_B) + \overline{\beta\gamma}\,(x_C - x_B)$$

$$\text{C} \,.\; {}_{2t}x_C = \overline{\beta\gamma}\,(x_B - x_C) + \overline{\gamma\delta}\,(x_D - x_C)$$

. . . .

. . . .

$$\text{K} \,.\; {}_{2t}x_K = \overline{\iota\kappa}\,(x_I - x_K) + \overline{\kappa\lambda}\,(x_L - x_K)$$

$$\text{L} \,.\; {}_{2t}x_L = \overline{\kappa\lambda}\,(x_K - x_L) + \overline{\lambda\mu}\,(x_M - x_L)$$

$$M \,.\, {}_{2t}x_M = \overline{\lambda\mu}\,(x_L - x_M) + \overline{\mu\mu}\,(x_M - x_M) \quad . \quad . \quad (37.)$$

where A is supposed to be the first; and M the last body in the series. For the sake of preserving uniformity in appearance, zero has been added to the first and last equations under the forms $\overline{\alpha\alpha}\,(x_A - x_A)$ and $\overline{\mu\mu}\,(x_M - x_M)$, $\overline{\alpha\alpha}$ and $\overline{\mu\mu}$ being the coefficients of elasticity of two arbitrary springs, which having no points of attachment, have, in reality, nothing to do with our present inquiry.

Now, I have shown in the preceding section, that the motions of any system of bodies connected by pressures proportional to their distances, are composed of as many oscillations, less one, as there are bodies; and the application of the same reasoning to the present case is so obvious, that it is quite needless to take up time with the repetition of it. These various oscillations do not interfere with each other, so that if one oscillation which is consistent with the conditions of a system be superadded to another oscillation which is consistent with the same conditions, the compound motion will also be so.

We are therefore at liberty to assume, that one of the constituent vibrations is represented by equations of the form

$$x_A = a\mathrm{U} \sin(\theta t + u)\,;\quad x_B = b\mathrm{U} \sin(\theta t + u)\,;\ \&c.,$$

in which U is the half extent of the oscillation of the nucleus; u an interval of time, which gives the epoch of the oscillation; θ a multiplier, which gives the rapidity; and a, b, c, &c., coefficients which determine the particular shares of that vibration which belong to the several bodies.

Inserting these values of x_A, x_B, &c., in equations (37), and dividing all by U sin $(\theta t + u)$, there result the following equations:—

$$
\begin{aligned}
-aA\theta^2 &+ \overline{\alpha\alpha}\,(a - a) + \overline{\alpha\beta}\,(b - a),\\
-bB\theta^2 &= \overline{\alpha\beta}\,(a - b) + \overline{\beta\gamma}\,(c - b),\\
-cC\theta^2 &= \overline{\beta\gamma}\,(b - c) + \overline{\gamma\delta}\,(d - c),\\
&\quad . \quad . \quad . \quad . \quad . \quad . \quad .\\
&\quad . \quad . \quad . \quad . \quad . \quad . \quad .\\
&\quad . \quad . \quad . \quad . \quad . \quad . \quad .\\
-kK\theta^2 &= \overline{i\kappa}\,(i,\ k) + \overline{\kappa\lambda}\,(l,\ k),
\end{aligned}
$$

$$-lL\theta^2 = \overline{\kappa\lambda}\,(k, l) + \overline{\lambda\mu}\,(m - l),$$

$$-mM\theta^2 = \overline{\lambda\mu}\,(l - m) + \overline{\mu\mu}\,(m - m)\,; \quad . \quad (38.)$$

which give, by addition, the equation

$$aA + bB + cC + dD + \&c., = 0 \quad . \quad . \quad . \quad . \quad (39.)$$

The equations (38) may be put in the form

$$-\frac{\overline{\alpha\alpha}}{\overline{\alpha\beta}}a + \frac{\overline{\alpha\alpha} + \overline{\alpha\beta} - \theta^2 A}{\overline{\alpha\beta}}a = b,$$

$$-\frac{\overline{\alpha\beta}}{\overline{\beta\gamma}}a + \frac{\overline{\alpha\beta} + \overline{\beta\gamma} - \theta^2 B}{\overline{\beta\gamma}}b = c,$$

$$-\frac{\overline{\beta\gamma}}{\overline{\gamma\delta}}b + \frac{\overline{\beta\gamma} + \overline{\gamma\delta} - \theta^2 C}{\overline{\gamma\delta}}c = d,$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$

$$-\frac{\overline{\kappa\lambda}}{\overline{\lambda\mu}}k + \frac{\overline{\kappa\lambda + \lambda\mu} - \theta^2 L}{\overline{\lambda\mu}}l = m,$$

$$-\frac{\overline{\lambda\mu}}{\overline{\mu\mu}}l + \frac{\overline{\lambda\mu} + \overline{\mu\mu} - \theta^2 M}{\overline{\mu\mu}}m = m\,; \quad . \quad . \quad (40.)$$

From these equations, it is seen that the series of quantities a, a, b, c . . . . . . k, l, m, m closely resembles a Brounckerian progression; also, from the very nature of the equations, the quantities a, b, c, &c., may all be changed in any ratio, since that would merely cause a change of the value of U in the inverse ratio. In other words, we may arbitrarily assume any one of these, and thence deduce the values of the others. Having then assumed a, we can go on deducing successively the values of b, c, d, &c., in terms of the unknown quantity θ^2; and then, in order to satisfy the conditions of the system, the value of m deduced from the penult equation must agree with that deduced from the last one.

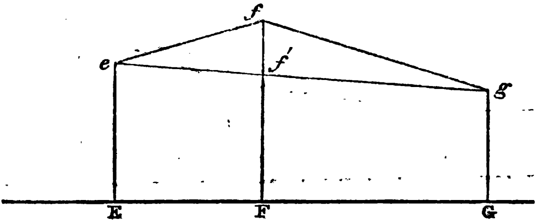
If we can discover that value of θ^2, which conducts us from two equal values a and a to two other equal values m and m, the same value of θ^2 will conduct us back from m and m to a and a. Whence we arrive at this conclusion, that if the series be repeated in inverse order, so as to make a double system,

$$a, b, c \;.\;.\;.\; k, l, m, m, l\; k \;.\;.\;.\;.\; c, b, a\,;$$

every value of θ which suits the original series will also suit the double one. In the same way, if a third, fourth, fifth, &c., series be annexed, the vibrations of them all may go on simultaneously, and as if they were detached. The elasticities of the connecting springs $\overline{\alpha\alpha}$ and $\overline{\mu\mu}$ are never brought into action; since $x_{\text{A}}=x_{\text{A}}$ and $x_{\text{M}}=x_{\text{M}}$; wherefore, a simple oscillation in the first series could never have communicated a simple oscillation of the same kind to the adjoining part: since a compression or distension of the springs represented by $\overline{\mu\mu}$ and $\overline{\alpha\alpha}$ is incompatible with the existence of any vibration in the system A, B . . . . L, M.

If two portions of different lengths be taken from a uniformly stiff helical spring, their coefficients of elasticity are inversely proportional to those lengths. Hence the arrangement of the system of bodies A, B, C, &c., may be represented by assuming an imponderous uniform continuous spring, and by marking off portions AB, BC, ... KL, LM, inversely proportional to the coefficients $\overline{\alpha\beta}$, $\overline{\beta\gamma}$; $\overline{\kappa\lambda}$, $\overline{\lambda\mu}$. At these points we have to imagine the bodies A, B, . . . L, M, securely fixed to the spring, in order to obtain a graphic representation of the system when in a state of rest. For the sake of uniformity, we shall annex at each end the arbitrary portions 'AA and MM', observing that the magnitudes of these have no real part in our investigation.

This much being arranged, let the perpendiculars A*a*, B*b* . . . . L*l*, M*m* represent the coefficients of U sin $(\theta t+u)$ belonging to the several bodies. Our first business is to examine how these are successively derived, on the supposition that the value of θ is known.



Let E, F, G be the positions of the three bodies E, F, and

G, upon such an elastic line; and let E*e*, F*f*, G*g*, be the three coefficients *e*, *f*, *g*; then, according to equations (38), observing that EF and FG take the places of $\frac{1}{\varepsilon\zeta}$ and $\frac{1}{\zeta\eta}$, we ought to have

$$\frac{\mathrm{E}e}{\mathrm{EF}}+\left\{\theta^2\mathrm{F}-\frac{1}{\mathrm{EF}}-\frac{1}{\mathrm{FG}}\right\}\mathrm{F}f+\frac{\mathrm{G}g}{\mathrm{FG}}=0 \quad . \quad . \quad (41.)$$

or $$\mathrm{E}e\,.\,\mathrm{FG}+\{\mathrm{EF}\,.\,\mathrm{FG}\,.\,\theta^2\mathrm{F}-\mathrm{EG}\}\,\mathrm{F}f+\mathrm{G}g\,.\,\mathrm{EF}=0.$$

Now, if we draw the straight line *eg*, cutting the intermediate ordinate in *f*′,

$$\mathrm{E}e\,.\,\mathrm{FG}-\mathrm{EG}\,.\,\mathrm{F}f'+\mathrm{G}g\,.\,\mathrm{EF}=0 \quad . \quad . \quad . \quad (42.)$$

wherefore, subtracting

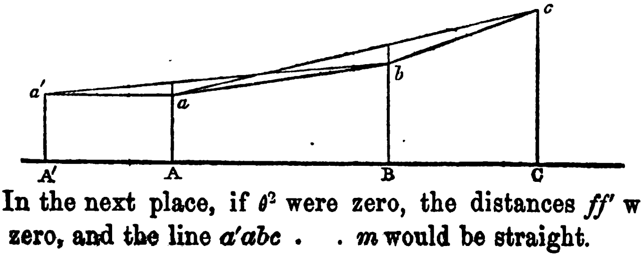
$$\mathrm{EF}\,.\,\mathrm{FG}\,.\,\theta^2\mathrm{F}\,.\,\mathrm{F}f=\mathrm{EG}\,.\,ff'; \text{ or,}$$

$$ff'=\frac{\mathrm{EF}\,.\,\mathrm{FG}}{\mathrm{EF}\,.\,\mathrm{FG}}\,\theta^2\mathrm{F}\,.\,\mathrm{F}f. \quad . \quad . \quad . \quad . \quad (43.)$$

So long, then, as θ^2, which has taken the place of the $-$R of our first investigation, is positive, the point *f*′ must be towards the line of abscissæ, and therefore *efg* must be concave towards EFG.

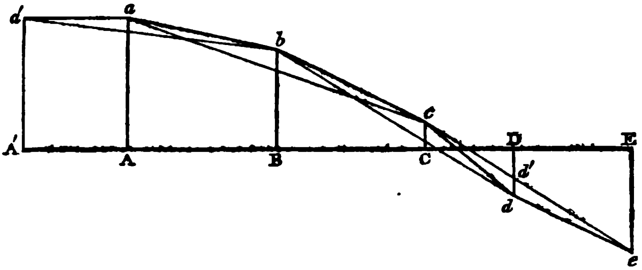
In order that the system A, B, C . . . K, L, M may vibrate without any extraneous influence, it is necessary that the ordinates beyond A*a* and M*m*, should be respectively equal to A*a* and to M*m*; therefore, we have to discover such a value of θ^2, that having started from the two equal ordinates A′*a*′ and A*a*, at the one end, we may reach two equal ordinates, M*m* and M′*m*′, at the other end of the series.

Now, in the first place, if R had been positive, the distances *ff*′ would have been measured *from* the line of abscissæ; and therefore, none but the two first could ever have been equal to each other.



In the next place, if θ^2 were zero, the distances *ff*′ would be zero, and the line *a*′*abc* . . . *m* would be straight.

On supposing θ^2 gradually to increase from zero, the line *a'abc* . . . from being parallel to A'ABC . . . would bend down more and more, and would at last come to cross the axis.



Whenever any ordinate, as D*d*, has come to be negative, the portion *dd'* of it, determined by the formula

$$dd' = \frac{\mathrm{CD}\ .\ \mathrm{DE}}{\mathrm{CD}+\mathrm{DE}}\ .\ \theta^2\mathrm{D}\ .\ \mathrm{D}d$$

must have the same sign; and, therefore, the line *cde* is again concave to the axis, wherefore it appears, that the equality of M'*m'* to M*m* can only occur after the line *abc* . . . has crossed the axis.

As the value of θ^2 gradually augments, the line *a'abc* . . . after having crossed the axis and begun to bend upwards, may come to give M*m*=M'*m*; this value of θ is the least root of equation (36), and corresponds to the slowest vibration of which the system A, B . . . L, M is capable.

By continuing still to augment the value of θ^2, the line *a'abc* . . . may be made to recross the axis, and, in the course of the gradual change, the two ordinates M*m* and M'*m'*, this time above the axis, will come again to be equal. The value of θ^2 then, is the second root of equation (36), and gives the vibration second in point of length. Thereafter, the increase in the value of θ^3 will give more and more numerous crossings, with intermediate cases of M*m*=M'*m'*; but no value of θ^2 however great, can give more than one crossing in each interval,—that is to say, more than $n-1$ crossings in all; and thus we see, that equation (36) has all its roots positive and real.

Every linear elastic series, then, is capable of as many distinct simple vibrations as it has constituent parts less one; and it is capable of no other simple vibrations; therefore, every internal motion of a linear elastic series, not subjected to extraneous influences, may be represented by the formula

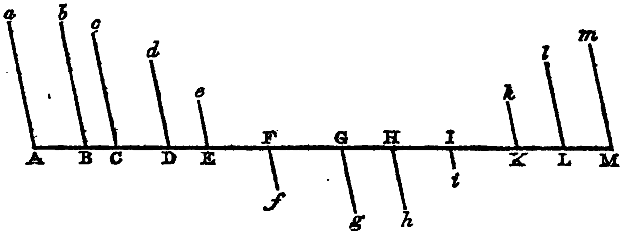
$$x_\nu = \Sigma\,\{f_\nu U_\nu \sin\,(t\theta_\nu + u_\nu)\}\;; \quad . \quad . \qquad (44.)$$

in which the sum is to be taken for every value of ν from 1 to $n-1$ inclusive.

One of these separate vibrations may be thus illustrated:—

Let the ordinates A*a*, B*b* . . . L*l*, M*m* be supposed to be jointed at A, B . . . L, M, and to be by mechanical means preserved parallel to each other. If one of them, as A*a*, oscillate backwards and forwards, so that the projection of *a* upon the axis oscillate according to the law $x_A = a\mathrm{U}\sin(\theta t)$, then the projections of all the other points *b*, *c*, &c., will oscillate in such a way as to represent the motions of the bodies B, C, &c.

Since we have $a\,.\,\mathrm{A} + b\,.\,\mathrm{B} + \&c. = 0$, it follows that if all



the masses A, B . . . L, M, were transferred to the points *a*, *b* . . . *l*, *m*, the position of their common centre of gravity would not be altered, in whatever direction the lines A*a* . . . M*m* are placed, provided they be all parallel to each other, and thus the position of the centre of gravity of the system is not influenced by any of its vibrations; which is in accordance with the universal law, that the internal actions of any system do not affect the motion of its centre of gravity.

The quantities f_ν and θ_ν of equation (44), are deduced from the data of the system, and are therefore invariable; the quantities U_ν and u_ν, on the other hand, have to be determined to suit the circumstances of the impulse which was originally given; that is, to suit the positions and velocities of the several bodies at a given epoch, as at the commencement of the time t. Wherefore, if (x_A) represent the position, and (v_A) the velocity of the body A at the instant of time $t=0$, we must have the equations

$$(x_{A})=\Sigma\{a_{\nu}U_{\nu}\,.\,\sin u_{\nu}\} \quad . \quad . \quad (46.)$$

$$(v_{A})=\Sigma\{\theta_{\nu}a_{\nu}U_{\nu}\,.\,\cos u_{\nu}\} \quad . \quad . \quad (47.)$$

If, then, the state of the system at the epoch $t=0$ be completely given, we shall have as many equations (46) and (47) as may serve to determine the whole of the quantities U and u; and from these the positions of the various parts at any future time.

On the Colouring Matter of Persian Berries. By JOHN GELLATLY, Assistant to Dr ANDERSON, College Laboratory, Glasgow.*

Several species of the genus *Rhamnus* have been examined by chemists, and all have been found to contain yellow colouring matters, which, so far as our present knowledge goes, appear to be different. The *Rhamnus catharticus* has been investigated by Fleury,† the *Rhamnus frangula* by Buchner‡ and Casselmann,§ and the *Rhamnus tinctoria* by Kane.‖

Two varieties of the seed of the latter plant are found in commerce, known by the names of Persian and Turkey berries, the former being considered superior to the latter. Both of these have been examined by Kane, who considers the former to be unripe and carefully dried, the latter ripe and ill-preserved. He gives the following account of the properties of these substances:—

"I have found the unripe berries of the *Rhamnus tinctoria* (Persian berries, grains d'Avignon) to contain a substance soluble in alcohol and ether, and crystallizing from its ethereal solution in minute silky needles of a brilliant yellow colour; it gives, with metallic oxides, yellow lakes. When cautiously heated it is not volatile. In the ripe berry this substance, to which I have given the name *Chrysorhamnine*, is totally replaced by another, which I term *Xanthorhamnine*, which is of a much less beautiful yellow, and does not crystallize; this change is effected also by boiling the chrysorhamnine for a few minutes with water, or by contact with alkalies. The xanthorhamnine is totally insoluble in ether, but easily soluble in alcohol and water. It is formed by the union of the elements of water with chrysorhamnine. Its silver salt is yellow when first thrown down, but rapidly becomes black, metallic silver separating, and a colourless organic substance being formed. The Persian berries are much used for dyeing yellow, but from the processes employed, the xanthorhamnine alone is actually brought into play."

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* Read before the Royal Society of Edinburgh, 15th March 1858.

† Journal de Pharmacie, vol. xxvii. p. 666.

‡ Annalen der Chemie und Pharmacie, lxxxvii. p. 218.

§ Annalen der Chemie und Pharmacie, vol. civ. p. 77.

‖ Philosophical Magazine (3) vol. xxiii. p. 3.

The Persian berries which I examined were very different from Kane's, for, on digestion with ether, they yielded only a small quantity of a greenish resin, and no chrysorhamnine; but alcohol extracted a considerable quantity of a yellow substance easily obtained in fine crystals, in which respect it differs from Kane's xanthorhamnine, although I believe it to be that substance in a higher state of purity than that in which Kane obtained it, and have therefore retained his name.

Xanthorhamnine is prepared by digesting the coarsely ground berries for a short time with boiling methylated spirit, filtering and expressing the residue. The fluid, on standing for twenty-four hours, deposits a considerable quantity of a dark brown resin, from which it is poured off and again allowed to stand, and this is repeated as long as resin deposits. After some days crystals begin to make their appearance, and gradually increase until the fluid is converted into a semisolid mass. The rapidity of this change appears to depend to a great extent upon the concentration of the fluid, and it takes place best when it is not too strong. Agitation for a quarter of an hour occasionally produces crystals abundantly; but the solid matter separated in this way is very impure, and it is better to allow them to deposit slowly.

The dark mother liquor being pressed out, the substance is purified by three or four crystallizations from alcohol. It separates from the solution much more readily a second time, and, when nearly pure, deposits as the alcohol cools.

Xanthorhamnine appears in dense tufts of silky needles of a pale yellow colour, and nearly tasteless. They dissolve readily in both cold and hot water; but no crystals are obtained from the aqueous solution. It dissolves in cold, and very readily in warm alcohol; and if the hot solution be highly concentrated, the xanthorhamnine deposits in the form of a pale yellow semifluid resin, resembling turpentine in consistence, but which becomes crystalline if left standing with fresh alcohol above it. It is quite insoluble in ether, even on boiling. The following analyses were made on the substance dried at 212°. They are of three separate preparations :—

I. 7·493 grains substance gave
14·404 ... carbonic acid,
3·942 ... water.

II. 6·811 grains substance gave
13·045 ... carbonic acid,
3·510 ... water.

III. 7·874 grains substance gave
14·960 ... carbonic acid,
4·070 ... water.

IV. 6·817 grains substance gave
12·975 ... carbonic acid,
3·654 ... water.

	I.	II.	III.	IV.
Carbon,	52·43	52·24	51·82	51·91
Hydrogen,	5·85	5·58	5·74	5·95
Oxygen,	...	...	...	...

Kane assumes $C_{23}H_{13}O_{15}$ as the formula of the substance dried at 212°, and $C_{23}H_{12}O_{14}$ for that dried at 320°, which, however, do not agree in a very satisfactory manner with his experimental results, as may be seen by the subjoined numbers.

	Calculation.	Experiment.
C_{23} .	52·67	52·55
H_{12} .	4·58	5·15
O_{14} .	42·75	...

Independently of the odd number of equivalents of carbon, the hydrogen found by experiment is too high for his formula, and my results being still higher than his, render it necessary to propose another. The decomposition to be afterwards referred to give for it the formula $C_{46} H_{28} O_{28}$, which agrees very well with the experimental numbers, as is seen by the following comparison:—

	Experimental Mean.	Theory.		
Carbon,	52·10	52·27	C_{46}	276
Hydrogen,	5·78	5·30	H_{28}	28
Oxygen,	42·12	42·43	O_{28}	224
	100·00	100·00		528

The air-dried substance contains, besides, ten atoms of water, as shown by two different determinations on different preparations.

I. { 5·593 grains substance lost
·793 ... at 212°.

II. { 9·698 grains substance lost
1·395 ... at 212°.

I.	II.	Theory.
14·36	14·38	14·56

The formula of the crystals is therefore $C_{46}H_{28}O_{28}$, 10 HO. These crystals do not fuse in the water-bath, but give off all their water. They remain solid even when heated to 300°, in which respect they differ from Kane's substance, which, fused under 212°, and continued to lose water until the temperature rose to 350°, at which point decomposition commenced.

That this substance was really the colouring matter of the berries was shown by dyeing a piece of mordanted cloth with it, when a fine yellow was got with the alumina, and a black with the iron portion.

It forms bulky yellow lakes with the oxides of tin, lead, and alumina, which cannot be obtained of definite composition without much difficulty. The lead compound, which is the most easily obtained, was prepared by adding a solution of neutral acetate of lead to an alcoholic solution of the colouring matter keeping the latter in excess. It is a pale yellow and somewhat granular precipitate, which acquires an orange colour on the addition of excess of lead salt. The excess of colouring matter seems to adhere to it rather obstinately, and it is only by careful washing that it can be obtained in a state fitted for analysis. It is somewhat soluble in water, and not very insoluble in alcohol. Dried at 212°—

6·905 grains substance gave
1·850 ... oxide of lead.

5·505 grains substance gave
7·610 ... carbonic acid,
2.220 ... water.

Calculation.			I.	II.
C_{46}	276	36·75	...	37·70
H_{28}	28	3·73	...	4·08
O_{28}	224	29·82	...	...
2 PbO	23·12	29·70	26·79	...
	751·12	100·00		

The air-dried substance appears to contain 8 atoms water, as 7·56 grains lost ·655 grains at 212°.

Experiment, 8·66 . Theory, 8·75

approximating the formula $C_{46}H_{28}O_{28}$. 2 PbO, 8 HO.

Xanthorhamnine gives brown solutions with alkalies, which become pale on the addition of acids. From an alcoholic solution, caustic potass separates a hard reddish resin, and the alkaline earths give yellow precipitates in not too dilute solutions. When the colouring matter is boiled for some time with baryta water, a red substance separates, which, on exposure to the air, immediately becomes quite black. Solutions of iron produce a black colouration.

Chlorine and bromine give dark resinous products with a watery solution. Nitric acid oxidizes it, yielding a red fluid containing oxalic acid. Strong sulphuric acid dissolves the colouring matter, giving a yellow precipitate on dilution with water.

When xanthorhamnine is boiled with dilute sulphuric or hydrochloric acid, a pale yellow matter immediately separates, and on filtering this off, and testing the filtrate with sulphate of copper, grape sugar was easily detected, showing that the colouring matter is a glucoside. In the proportion of its constituents, its softish, nearly tasteless, crystals, and insolubility in ether, it agrees with these bodies generally.

The pale yellow matter separated by boiling with the dilute mineral acids, according to the nomenclature in use for this class of substances, should receive the name of Xanthorhamnetine, but for convenience I propose to drop the prefix and call it simply Rhamnetine.

A very few minutes suffices to separate the greater portion of the rhamnetine; but the last traces cannot be obtained without protracted boiling. Metallic salts have a singular tendency to retard this decomposition. If an acidified solution, for instance, be divided into two portions, and sulphate of zinc added to one of them, the latter requires much longer boiling before the rhamnetine shows itself. It is almost insoluble in water, alcohol, and ether; alkalies dissolve it, and acids re-precipitate it from the solution. Three separate preparations were analyzed, with the following results:—

I.	3·665	grains	substance dried at 212° gave
	8·010	„	carbonic acid,
	1·435	„	water.
II.	5·348	grains	substance gave
	11·640	„	carbonic acid,
	2·040	„	water.
III.	5·255	grains	substance gave
	11·420	„	carbonic acid,
	2·150	„	water.

	I.	II.	III.
Carbon,	59·61	59·36	59·27
Hydrogen,	4·35	4·24	4·55
Oxygen,	...	...	...

These numbers agree with the formula $C_{22}H_{10}O_{10}$, as is seen by the following comparison of the experimental mean with the calculation:—

	Mean.	Calculation.		
Carbon,	59·41	59·46	C_{22}	132
Hydrogen,	4·38	4·50	H_{10}	10
Oxygen,	36·21	36·04	O_{10}	80
	100·00	100·00		222

On adding the formula of rhamnetine to that of grape sugar, we have xanthorhamnine plus six atoms of water—

$$\underset{\text{Xanthorhamnine.}}{C_{46}H_{28}O_{28}} + 6HO = \underset{\text{Glucose.}}{C_{24}H_{24}O_{24}} + \underset{\text{Rhamnetine.}}{C_{22}H_{10}O_{10}}$$

The accuracy of this view was further proved by ascertaining the quantity of rhamnetine yielded by a given quantity of the original substance, when 32·91 grains gave 13·60 grains, equal to 41·3 per cent.; theory requires 42 per cent.—the loss is attributable to the slight solubility of the rhamnetine. A sugar determination by Fehling's method, gave 68·9 per cent.; theory 68·2.

Kane gives for chrysorhamnine the formula $C_{23}H_{11}O_{11}$; his numbers are,—

	Theory.	Experiments.	
23 Carbon,	58·23	58·23	57·81
11 Hydrogen,	4·64	4·77	4·64
11 Oxygen,	37·13	37·00	37·55
	100·00	100·00	100·00

If this formula be doubled, a simple relation is seen to exist between it and the xanthorhamnine I have described.

$$\underset{\text{Chrysorhamnine.}}{C_{46}H_{22}O_{22}} + 6HO = \underset{\text{Xanthorhamnine.}}{C_{46}H_{28}O_{28}}$$

As chrysorhamnine is converted into xanthorhamnine by boiling for a few minutes in water, the above equation satisfactorily explains the change. The further examination of chrysorhamnine is most desirable, as the previous history of any glucoside during its formation in the plant is quite unknown. I may state that I have examined several samples of Persian berries, but have failed to obtain it, even when they appear likely to yield it.

The only other point to be noticed is, that when the entire berries are stirred up with water and filtered, they give a solution which immediately begins to deposit a large quantity of a yellow powder, having the character and composition of rhamnetine, as shown by the following analysis :—

$$\left\{\begin{array}{lll} 4·93 & \text{grains} & \text{substance gave} \\ 10·64 & ,, & \text{carbonic acid,} \\ 2·00 & ,, & \text{water.} \end{array}\right.$$

	Experiment.	Theory.
Carbon, .	58·86	59·46
Hydrogen, .	4·51	4·50

The solution is feebly acid, but the appearance of this precipitate is not easily explained, as the pure colouring matter requires boiling with acids before it is decomposed, while this comes down spontaneously from a cold solution of the berries.

The preceding pages form a small contribution to our knowledge of the colouring matter of the *Rhamnus tinctoria.* It is beyond a doubt that some species of the genus contain compounds of an entirely different character from that found in Persian berries. Franguline, the crystalline product of *Rhamnus frangula,* is certainly not a glucoside; but of the substance obtained from the *Rhamnus cathartica,* too little is known to enable us to pronounce upon its chemical nature, although it seems to be different from all the others. The whole subject wants further inquiry.

In conclusion, I have to thank Dr Anderson for the use of his laboratory, and also for his advice during the progress of this examination.

On the Fall of Rain in Scotland during the year 1857; *with remarks on the best form of Rain-gauge, and the position in which it ought to be placed; and on the causes which appear to influence the deposit of Rain in different localities.* By JAMES STARK, M.D., F.R.S.E., V.P.R.S.S.A., Secretary to the Meteorological Society of Scotland.*

It is to the want of a knowledge of the amount of rain which falls in different localities that the failures in draining land are for the most part to be attributed. An engineer accustomed to drain land situated in a low-lying level country, by placing his drains at the distance of 30 or 40 feet from each other, finds he does not clear the land of its superabundant moisture in another locality by placing his drains at the same distance, although the soil and subsoil appear to be the same, and in his eyes the situation seems not very dissimilar. Hence has arisen that great diversity of opinion and of practice among agricultural engineers which we find at the present day. All, however, are now beginning to look to the rain-fall as the probable cause of this want of success; and as the drains must be put so close as to carry off the rain which falls on the land, and is not required for the growth of the plant, or is lost by evaporation from its surface, it is now acknowledged, that just in proportion to the amount of rain which falls in a district must the drains be put close to each other. Agricultural engineers are therefore anxiously looking to meteorologists to guide them to some principles which will enable them to guess at the probable rain-fall in any district to which they may be called.

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* Read before the Royal Scottish Society of Arts, March 22, 1858.

If we turn to any work on Meteorology, we shall find that the most vague statements are made relative to the causes which influence the deposit of rain in any locality, and no principles are laid down which can guide the agricultural engineer in his drainage operations. True it is that these works do mention many of the causes of this deposit; but this is done in such a manner as to show that the writers themselves had no clear idea of the mode in which these varied causes operated, nor how they were connected with, or mutually influenced, each other.

Mr Denton, engineer to the Land Drainage and Improvement Company, painfully experiencing the want of some guide to the rain-fall in the different districts of Great Britain, in a very able paper which he read before the Society of Arts in London, propounded a scheme for dividing the country into squares of 5 or 10 miles, appointing a person in each square to register the daily fall of rain, and have the whole returns classified and published at some head office. This, he calculated, could be done for about L.26,000 per annum. The scheme is far too expensive ever to be thought of, either by Government or by private enterprise; and the whole practical results which are desired could be obtained much more easily and surely, and at an expense to Government of a very few hundred pounds yearly, through the Meteorological Societies of England and of Scotland.

It is only for the two past years that Scotland has had the advantage of having the meteorological phenomena, which were observed at different parts of the country, collected, classified, and reduced to a tangible form. Of these returns not the least valuable are those of the fall of rain at each of the different stations; and as every care has been taken to secure, as far as practicable, the employment of the same form of gauge, and to place that gauge in a like position, and as all the observers whose results are printed are thoroughly competent to the task, their observations may be depended upon.

The rain-gauges in common use over Scotland are those known by the name of that distinguished naturalist, who has just passed from us, the late Professor Fleming. These gauges consist of two copper cylinders, of which the one fits within the other. The outer cylinder is sunk in the ground to within an inch of its top, in a spot covered with close-shaven grass. The inner cylinder, which is 3 inches in diameter, has a projecting rim on its outer surface, about 3 inches from its top, which overlaps the junction of the two cylinders, and prevents water finding its way into the outer cylinder which is sunk in the ground. The throat of the inner cylinder is nearly closed by means of a funnel-shaped diaphragm, through which passes the stem of the hollow copper float; and a stop-cock is fixed to the bottom of this cylinder, to allow the water which collects to be drawn off, and weighed or measured if considered desirable. The stem of the float is divided into inches and tenths of inches; and in using the instrument enough of water is added to float the copper float, and raise the stem so far that the cross-bar cuts the stem exactly at the zero point. The smallest fall of rain can thus be easily read off by drawing out the inner cylinder, and bringing the graduated stem to the level of the eye; or, if still finer readings are wanted, the water is drawn off by the stop-cock till the stem is lowered to the zero point, and is measured in a graduated glass vessel. After having seen much of the working of this form of gauge, I have no hesitation in saying that I prefer it to most others. The most minute readings can be got by drawing off the water by the stop-cock daily, and measuring it in a graduated glass jar; and its non-liability to get out of order, its moderate price, and easy management, as well as the circumstance of the float preventing loss by evaporation, are no small recommendations to its general employment.

From experiments which were made, it was ascertained that there was no advantage in exposing a large surface for the reception of the rain; and that this 3-inch gauge, when placed alongside of others of larger size and different construction, but all on the same level and exposure, collected a depth of rain equal to that of any other gauge. On placing the gauge at various elevations, the fact which has been long known was also observed, that the quantity of rain collected in the gauge which was on a level with the ground was much larger than that collected at the height of 4, 6, or 10 feet above the surface. The true quantity of rain which fell on the ground (and that is the important point both for agriculturists and meteorologists) was invariably best ascertained by having the rain-gauge placed as close to the ground as possible.

It is perfectly surprising to observe the number of hypothetical explanations which have been given in the attempt to account for this excess of rain collected by rain-gauges placed on the surface of the ground. Even Sir John Herschell, in his Essay on Meteorology, published last year in the "Encyclopædia Britannica," says on this point, "The real cause is yet to seek, and there is no more interesting problem which can fix the attention of the meteorologist." And he then adds, as his own hinted explanation,—"visible cloud rests on the soil at low altitudes above the sea but rarely; and from such clouds only would it seem possible that so large an accession of rain could arise."

Little trouble was taken by me to investigate the cause of this supposed mysterious agency of nearness to the ground in inducing a greater deposit of rain in the gauge; a few visits to gauges at different elevations, *during storms of wind and rain*, convinced me that the agency was a very simple one indeed. It was the wind which made all the difference; and just in proportion as the gauge, *when raised above the ground*, occupied a situation more or less exposed to the force of the wind, in the same proportion did that gauge catch less or more of the rain which fell; so that while one four feet above the ground only caught one-tenth of an inch during a heavy thunder storm, which was accompanied by a driving wind, the gauge on the ground had collected upwards of half an inch.

Several of my correspondents have noticed the same fact, and all agree with me that the effect is produced by the wind. The swirl of wind round the mouth of the elevated gauge not only prevents the rain from falling into it, but even blows out of the funnel drops which have fallen into it. Until, then, the plan is universally adopted of placing the rain-gauge as near to the level of the ground as possible (*i.e.*, within four inches), in a place as free as possible from trees, houses, and walls, and surrounding the gauge with at least one foot in breadth of close-shaven turf, to prevent the rain-drops rebounding from the ground into the gauge, no comparable results can be obtained.

During 1856, which all acknowledge to have been a "wet" or "rainy" year, the mean fall of rain, as deduced from observations made at 37 stations in Scotland, was 37 (36·96) inches. During 1857, which all will equally acknowledge to have been a "dry" year, the mean fall of rain, as deduced from observations made at 55 stations, was 35 (34·9) inches. This single fact, then, shows that, to prove of any practical utility it is absolutely necessary to know much more about the rain-fall than the mere quantity of water deposited from the clouds. Even the knowledge of the average number of days on which rain fell will not help us: for we find that in 1856—the wet year—rain fell on 160 days; whereas in 1857—the dry year—rain fell on 163 days! In other words, the number of days on which rain fell was greater during the dry than during the wet year. But though rain fell during 1857 on three days more than it did during 1856, the general character of the showers, during the summer and autumn in especial, was quite different, and it was this which to no small extent constituted the difference between the character of the one year and that of the other. The showers during the summer and autumn of 1857 were heavy, but of short duration, and for the most part fell during the night; those of the summer and autumn of 1856 consisted chiefly of constant showers of drizzling rain, which kept ground and atmosphere in an unusually moist condition. This state of matters seemed to be principally produced by the kind of wind which prevailed during each of these periods. During August and September 1856 the rains principally came with a south-east and east wind, and were accompanied by that haziness which so often attends easterly winds during the summer and autumn months. In consequence of coming from the easterly side of the island, the quantity of rain which fell at stations on or near the east coast exceeded somewhat that which fell at west-coast stations similarly situated as to surrounding localities. During the corresponding months, however, of 1857, the rains chiefly came along with south-west or west winds, and though heavier for the time, were soon over, and rapidly dried up.

The rain-fall during 1857 presented several peculiarites as contrasted with that of 1856. Thus, March was the driest month in 1856, seeing only 0·37 of an inch of rain fell during that period; whereas, in 1857 least rain fell during May, yet the quantity was nearly two (1·85) inches. March, on the other hand, during 1857, instead of proving one of the driest, proved one of the wettest months of the year, no less than 3·41 inches of rain having fallen over Scotland during that period. In 1856, after March, the next driest months were October and November, the quantity of rain which fell being under two inches in depth (1·91 inches in October, and 1·98 inches in November) during each of these months; whereas in 1857, upwards of 2½ (2·57) inches of rain-fell during October, and upwards of 3 (3·05) inches during November. In 1856 the greatest depth of rain fell during December, and the next greatest quantity during September. In 1857, the greatest quantity of rain fell during September, and the next greatest depth in December. During both the wet and the dry year, therefore, September and December were the months when most rain fell; and the corollary I would draw from this fact is, that in farming operations the kind of seed selected for the grain crops should be such as shall either ripen early, and be cut down during August, before the September rains set in, or ripen late, so that they do not come to maturity till October. In either case, the chances of the crop being secured in good condition would be greater than if the grain harvest were interrupted by the rains when half of the crop was cut down.

It has long been taken as an established fact, that more rain falls on our western than on our eastern shores; and if we take inland stations near that coast, and contrast the fall of rain at them with the coast stations on the eastern side of the island, there can be no doubt of the fact. But I am clearly of opinion that this statement has done much to blind the eyes of meteorologists to the true state of the case, by inducing them to overlook the true cause, and ascribe to winds and adjoining seas what is truly due to the physical peculiarities of the country.

Now, what is the cause of rain? Speaking in a general way, we are accustomed to say, it is a deposit from the clouds, and so are thrown back to the inquiry as to what clouds are. Clouds consist for the most part of aqueous vapours in a state of partial deposition, which remain suspended, we know not how, in the air. In fact, the vapour is in that exact state which we term fog or mist. The character of these clouds is, however, very different,—some being peculiar to dry, settled weather; others to rainy weather. But it is not intended to enter into this subject at present. Any one who has lived in a mountain district may any day witness the manner in which clouds are formed; nay, we may see them forming in all situations every fine warm summer day. Whenever the warm air at the surface of the ground rises into the higher regions of the atmosphere it loses part of its temperature, when, not being able to retain in solution the whole of the aqueous vapour which it held dissolved at a higher temperature, the excess of vapour assumes that visible form which we term cloud or mist; and, if formed in this manner during summer, it takes the form of those large, rounded, lazily-moving masses termed the *cumulus* cloud.

Anything, however, which will reduce the temperature of a warm current of air will cause it to deposit its superfluous moisture in the form of a cloud, or, if the reduction of temperature be great, in the form of rain. Our chief rains come to us with the westerly or south-westerly winds, which blow over the Atlantic, and arrive at our shores loaded with moisture. If this warm current of air therefore should, in the upper regions of the atmosphere, encounter the cold counter-current from the north or east, its temperature will be reduced, and as it is thus no longer able to hold in solution, at this lower temperature, that quantity of moisture which it retained when its temperature was higher, the redundant vapour is deposited in the form of a cloud; or, if the quantity of moisture which is set free be great, that excess will fall in the form of rain. This cause, however, of the deposition of rain, though in constant operation, and very visibly seen in action during storms, is not that one which chiefly conduces to produce the *difference in the amount* of rain deposited in different localities. This latter cause must be sought for in the *physical configuration of the land;* and it is to this point that I desire, in especial, to direct attention, as of the highest importance to the agricultural engineer.

When the warm westerly current of air reaches our western shores, if the temperature of the land be equal or superior to that of the current itself, and this is the case during the greater portion of the year, then no moisture, no rain, will be deposited on the land, merely because it is land on our western coasts. But if this current of air meets with some obstruction to its horizontal progress, as by encountering a range of hills or mountains, and is thereby thrown up more or less into the colder upper regions of the atmosphere, it is more or less chilled, and deposits its moisture in the form of rain.

Now, it so happens, from the peculiar configuration of our island, that the great ranges of hills approach much nearer our western than they approach our eastern shores; and as the first ranges of hills or mountains which this Atlantic atmospheric current meets throw it up more or less in proportion to their extent and height, so just in proportion is the deposit of rain on their opposite or eastern sides. It is not, then, the single circumstance of the station being on our eastern or western coasts which causes it to receive a less copious or a more abundant fall of rain, but the circumstance of its being nearer to or further away from hills, which would throw the current of air into the upper regions, and, by thus chilling it, cause it to deposit its moisture. If any station, therefore, on the west coast be chosen, near the sea-shore, and with no high hills near it, nor between the station and the sea, and the fall of rain there be compared with a station on the eastern coast in all respects similarly situated, I suspect it will be found that, if anything, the comparison will be rather in favour of the greater fall of rain at the eastern coast station.

It most unfortunately happens, for the full illustration of this most important point, that I have only succeeded in getting ten months' returns of the rain-fall at two stations on the western coast which are situated between the sea and the hills, these stations being the town of Ayr, in Ayrshire, and Galson, on the west side of the Isle of Lewis. The imperfection in the Ayr returns was caused by the removal of the Ordnance Survey Staff to Stirling; that in the Galson returns by the removal of the observer to another part of the island. The ten months' returns, however, from these two stations, on the purely western shore, may be compared with the same ten months' returns from three stations on the eastern coast, situated very nearly exactly similar with respect to hills, and the result is highly instructive.

		Ten Month's Rain, Jan. to Oct. inclusive, 1857.
West Coast,	Galson,	16·50
	Ayr,	18·19
East Coast,	Pittenweem,	18·16
	Arbroath,	20·31
	East Linton,	25·75

At Galson, there fell during the first ten months of 1857 only 16·5 inches of rain. At Ayr, during the same period, there fell 18·19 inches. On the east coast there fell at Pittenweem, during the same months, 18·16 inches of rain; at Arbroath, 20·31 inches; and at East Linton, 25·75 inches of rain during the same period. From this it appears that a station on the west coast, if free from the influence of hills, has no greater a deposit of rain than if it were situated on the east coast, or any other open level locality. The comparison of the fall of rain at the above-named stations is of peculiar value this year, seeing that all our great rains came from the west and south-west, and, notwithstanding of this, the fall of rain was greatest at the east-coast stations.

But the influence of hills or mountains in increasing the deposit of rain is very marked. Stornoway, which is still on the west coast of Scotland, but on the *eastern* side of Lewis, and separated from the Galson station by the central hilly ridge of that island, instead of exhibiting a fall of rain to the extent of only 16·5 inches in ten months, had no less a fall of rain during the same period than 35·88 inches, or just about double the amount which fell on the *western* side of the same island. To contrast with Ayr there is no station on the other side of the hills nearer than Wanlockhead, a station so surrounded by high hills, that the Atlantic breezes, in blowing over them, are greatly chilled, and consequently deposit so much moisture that there fell in 1857 no less than 57·9 inches of rain, and in 1856 the quantity amounted to 64·8 inches.

For further illustration of the same subject, let us take two stations in the same parallel of longitude, but only about twenty miles distant from each other—the one, however, in an open plain, the other at the very base of hills,—I mean Glasgow and Greenock. Glasgow lies in an open plain, having no hills so near to it as to influence the deposit of rain. Greenock, on the other hand, is situated at the north-eastern base of hills which rise to the south-westward of it to the height of upwards of 600 feet. The fall of rain at Glasgow during the year 1857 amounted to 33·65 inches, while at Greenock, during the same period, the amount of rain collected amounted to 52·62 inches.

Let us take another instance, to illustrate the same fact, from the district of Eskdale, in Dumfriesshire. In Eskdale the country is comparatively level from the Solway Frith to Canonbie, so that when the Atlantic winds blow up the Solway they meet with no hilly or mountain barrier till they have passed over Canonbie. In this respect, then, Canonbie resembles, to some extent, Ayr or Galson in situation. During 1857 the fall of rain at Canonbie amounted to 30·95 inches. Ewes is only some twelve miles from Canonbie, but higher up the vale, and is surrounded by high hills; and, as might have been expected, the fall of rain there was much greater, amounting to 44·10 inches during the year. Ettrick Pen is at the head of Eskdale. It forms one of the Hartfell group of mountains, and rises to a height of 2265 feet above the level of the sea, and the fall of rain on it during the same year amounted to 67·15 inches.

These few instances, therefore, may serve to illustrate the point to which I wish to direct attention, viz.,—the influence of hills and mountains in determining the amount of rain which falls in any particular locality. Wherever the station is situated, this influence may be remarked; but the quantity of rain which falls at the station appears to be dependent on which side of the mountain or hill the station may be placed, on the height of the mountain, its nearness to this or that side of the Island, and whether the chief falls of rain during any particular year are accompanied by easterly or westerly winds.

Thus, in 1856, the chief falls of rain during the third quarter of the year came from the south-east; and, as this south-easterly current met with no interruption to its course before passing over East Linton, in Haddingtonshire, only 34·95 inches of rain fell at that station during the year. But Yester, though only about ten miles to the south, is situated at the northern base of the Lammermuir Hills, which threw this south-easterly current into the upper regions, and thereby caused it to deposit more of its redundant moisture. The consequence was, that the fall of rain at Yester, during that year, amounted to 42·35 inches.

In so far, then, as Scotland is concerned, if we know the exact geographical relations of the station, we cannot go far wrong in our estimate of the average fall of rain. If the station is situated in a plain, or gently undulating country, most of which localities occur on the eastern side of the Island, the yearly fall of rain will range from 25 to 30 inches, or thereabouts; and the rain-fall at stations nearer the hills will vary from 30 to 70 inches, or more, depending on the height of the hills, their position relative to the wind's course, their proximity to the western or eastern side of the Island, and the prevalent direction of the winds. Mere elevation alone, independent of other circumstances, will not increase the quantity of rain, as is strikingly illustrated by the amount collected at Braemar (which is 1110 feet above the sea level), being only 32·69 inches during the year 1857. The reason of this is abundantly apparent. Before the westerly currents of air have reached that station, they have deposited the greater part of their superfluous moisture on the sides of the numerous high ranges of mountains which they have to cross before they reach that locality; so that, notwithstanding its elevation, it receives not much more rain than if it had occupied a situation in the plain below.

On the Tides in the Sound of Harris. By HENRY C. OTTER, Esq., R.N., Captain of H.M.S. Porcupine.* (Plates IV. and V.)

Tides.—The law of the tidal stream in the Sound of Harris is very remarkable, and does not appear to be influenced to any great degree by the wind. It may be generally stated, that in summer, in neap tides, the stream comes from the Atlantic during the whole of the day, and from the Minch during the whole of the night. In winter, the reverse takes place, the Minch stream flows during the day, the Atlantic during the night.

In spring tides, both in summer and winter, the stream comes in from the Atlantic during the greater part of the time the water is rising, but never exceeds 5¼ hours, and flows back into the Atlantic during the greater part of the fall of the tide.

The stream from the Atlantic is therefore denominated the flood stream, that from the Minch the ebb stream.

The rise and fall of the tide was found to be influenced much more by the force and direction of the wind than the moon's parallax. A strong S. or S.W. wind raises the water to equinoctial height, but produces a very poor ebb. The ebbing or falling tide takes 15 to 20 minutes longer than the flood. Where the water is confined by rocks and islands, such as inside of Strome, the Red Rock, &c., the velocity is nearly 5 miles an hour during springs, and not much less during neaps; but in other places it does not exceed 2 to 2½ miles an hour.

Summer.—The ebb stream commences at full and change 1¼ hours before high-water, or 5 A.M., and runs about 6 hours; it then gradually loses upon the time of high-water, so that at mean tides the ebb does not commence until an hour after high-water, and only runs for 4 hours; this lasts for one or two days, when the ebb stream is suddenly found running all night, and continues to do so from one day before the quarter to two days after. At the next mean tides the ebb is found commencing early in the morning, and gradually approaching the time of high-water.

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* Read before the Royal Society, 1st March 1858.

The flood stream commences at full and change 1¼ hour before low-water, and continues to do so for about 3 days; it then rapidly takes an earlier turn, until, at the quarter of the moon, it is found coinciding with the morning low-water, or 6 A.M., and continues to run flood the whole of the day until 8 P.M. The greatest velocity of this stream is in the forenoon, or whilst the tide is rising,—sometimes in the afternoon, about 3 hours after high-water. The stream is very sluggish; and, if blowing hard from the S. or S.W., a faint ebb stream will be felt for an hour, or an hour and a-half; after which the flood resumes its place, and continues rather longer than it would otherwise have done but for this.

Winter.—The ebb stream commences at full and change the same as in the summer, about 1¼ hours before high-water, or 5 A.M.; it then gradually gains upon the time of high-water, until the quarter of the moon, when the ebb commences 3¼ hours before high-water, or 8·15 A.M., and runs until 7 P.M. The greatest velocity of this stream was found to be about 3 hours after high-water, or 3 P.M.

The flood stream, after running all night in neap tides, has only a short duration in the forenoon of mean tides; but, as an approximate rule, the flood commences in the day-time about the time of the moon's transit.

The above remarks apply to the eastern side of the Sound.

In the middle and on the western side of Berneray, the law is modified, and in some places altogether different.

Narrows of Berneray (*Summer*).—At full and change the flood stream commences half an hour before low-water by the shore, and continues to run in that direction 5 hours—the greatest velocity being 2½ to 3 miles an hour.

The ebb stream turns an hour before high-water by the shore, and runs with the same velocity.

In neap tides there is from 8 to 9 hours' flood in the day-time, and not more than 2 to 4 hours' ebb.

In winter, in neap tides, there is from 2 to 4 hours' flood during the day.

Hermetray Group, as before mentioned, at all times receives the flood stream from the Minch, which turns three-quarters of an hour later than high-water by the shore.

Groay Group.—In spring tides, the flood stream, or the stream from the Atlantic, only runs for 2¼ hours after high-water, and then turns to the north; the greatest velocity is 1¼ miles an hour. Further to the northward the flood stream runs longer.

The diagram, which is appended, will give a close approximation to the turn and duration of the stream in the day-time during the summer and winter months; but at the equinoctial, when the change is about taking place, the table can only be depended on at full and change.

Explanation of Diagram. (Plates IV. and V.)

To find the time of high-water, look out the moon's A.M. meridian passage, for the day required, at the top of either table; and at the side, where the two lines intersect the black curve, the time of high-water will be found.

To find when the ebb and flood stream begins and ends, look out the moon's A.M. meridian passage at the top, as before, in the summer or winter table, according to the time of year, and the white space will show the duration of ebb, the shaded space the duration of flood.

Notes to Captain Otter's Paper on the Tides in the Sound of Harris. By James Stark, M.D. F.R.S.E.

An interesting subject of inquiry is the probable cause of the flow of the current through the Sound of Harris. As the tidal wave in its progress from the south flows up both sides of the Western Isles, as far as the Sound of Harris, at the same time, so that at both the eastern and western extremity of the Sound the time of high-water is attained at the same hour, it is evident that the peculiar flow of the current through the Sound cannot be due to the tidal wave. The circumstance of the stream flowing from the Atlantic into the Minch all day during the summer months, but during the winter flowing all day from the Minch into the Atlantic, suggests the idea that, *during summer*, the level of the Atlantic must be *higher* during the day than during the night; while, *during winter*, the level of the Atlantic must be *lower* during the day than it is during the night; in fact, that this peculiarity in the tidal current is somehow connected with the length of the day. The influence of the sun on the tides is known to all in the phenomena of what are termed "spring" tides, which occur when the sun and moon are in conjunction, or in opposition; that is to say, at the periods of new and full moon. But the phenomena described by Captain Otter are evidently to be ascribed to a different cause.

If we suppose that the sun exerts a strong attractive power over a large body of water like the Atlantic, which is undeniable, then we should expect that attraction to be greatest, and its effect in raising the level of the water most marked, when the sun was more immediately over that body of water. Taking it for granted that the sun's power of attraction is just in proportion to the length of time when it shines on any particular body of water, then the great mass of the Northern Atlantic in the same parallel of latitude as Harris, would have a higher level during the day in the summer months than it would have during the night when the sun's attractive power was removed. As the Minch is, to a certain extent, a confined sea, the current from the Atlantic would, therefore, flow into it all day; but when the level of the North Atlantic fell during the night, in consequence of the sun's attractive power being removed, the current would flow from the Minch into the Atlantic.

During winter, again, the sun's rays being most powerful over the Southern Atlantic, as it is now to the south of the equator, the waters of the North Atlantic would be attracted southwards during the day, so that its level would be lower than that of the confined waters of the Minch. Consequently, during the winter months, we should expect that the stream would flow through the Sound of Harris from the Minch into the Atlantic all the day. When the sun's attractive power, however, over the Southern Atlantic was removed during the night, the waters would fall to their level and allow the North Atlantic to regain its level; so that during the night the current during the winter season would flow through the Sound of Harris from the Atlantic.

On the supposition that this explanation is the true one, it appears to me that it throws light on a phenomenon which has been long remarked, but never satisfactorily accounted for,—viz., that during one period of the year the highest tides occur when the moon is *above* the horizon, but during the other half of the year when the moon is *below* the horizon. Now, if the moon be above the horizon during the summer when the level of the Atlantic is higher than usual from the greater attractive power of the sun, the day tide will be higher than the corresponding night tide. But if the moon be above the horizon during the day, when the Atlantic level is *below* its mean, as during winter, then the day tide will be lower than the corresponding night tide.

It would be interesting to ascertain, by actual measurements, whether there is any difference in the level of the waters in the Atlantic and Minch, and to what extent that difference exists during day and night, and during summer and winter; and I expect that this will be ascertained during the present year through the zeal of Captain Otter and Lieutenant Thomas, who are both engaged in the survey of the western coast.

Description of New Protozoa. By T. STRETHILL WRIGHT, M.D., Fellow of the Royal College of Physicians, Edinburgh.*

EXPLANATION OF PLATES.

Plate VI.

Fig. 1. *Lagotia viridis*, showing rotatory organ from lateral aspect.
2. Front view of do.
3. Tip of one of the lobes of rotatory organ—*a* large ciliary band—*b* striæ bearing cilia.
4. Young animal of *L. viridis*.
5. *Vagincola ampulla* (Müller).
6. *Vagincola valvata*, animal extended, and (7.) contracted — *a* valve raised—*b* do. closed.
8. Diagram of upper part of tube of *V. valvata*—*a* tube—*b* sarcode lining do.—*c* valve closed—*d* sarcode coating tube on outside.

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* Communicated to the Royal Physical Society of Edinburgh on the 25th April 1857.

Plate VII.

Fig. 1. *Ephelota coronata*—*a* with tentacles contracted—*b* with do. expanded.
2. Diagram of tentacle of *E. coronata.*

Family *Ophrydina*—Genus *Lagotia*.* (Mihi).

Lagotia viridis. (Plate VI., figs. 1, 2.) This remarkable member of the Ophrydina was discovered about two years ago, occurring in great profusion on a shell dredged up from the Firth of Forth. It rapidly multiplied itself until it studded the sides of the vessel in which it was kept, and various Algæ contained therein, with its dark-green cells. In March last it was again dredged from the same locality.

In general appearance the animal resembles Vagincola, though it differs from that genus in some important particulars. The cell resembles an amphora or flask lying on its side, having the neck bent more or less sharply upwards, and dilated into a trumpet-shaped mouth. Its colour is dark sea-green, in the larger specimens nearly opaque. The transparent green animalcule is long and cylindrical, as in other genera of the family Ophrydina, and is attached by its posterior extremity to the bottom of the tube. Its anterior extremity is crowned by a rotatory organ, the form of which is unique among the Protozoa, but which is the homomorph of the hippocrepian type, occurring in Alcyonella and others amongst the Polyzoa, and in Phoronis amongst the Annelida. This organ, when seen in front, and erect (fig. 2), appears like a narrow horse-shoe; whilst from the side the anterior extremity of the animalcule bears a resemblance to the head and ears of a hare. A thick muscular (?) band passes round the border of the horse-shoe, and forms the basis of a wreath of long vibratile cilia (fig. 3), the motion of which produces the optical illusion of moving cogs or teeth. The whole surface of the body and rotatory organ is seen (under a power of 300 diam.) to be striated with fine lines, which bear cilia in most active motion. The gullet (?) in the first specimen taken, was, in every case examined, a shallow sac placed within the bend of the horse-shoe and between the ciliary bands; but in the last batch of specimens, which were of much larger size, it invariably passed deeply within the body as a tapering canal, in which the motion of large cilia could be clearly detected.

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* λαγὸσ, a *hare*; ὠτίον, an ear-flap, an ear.

Although both colonies were exceedingly numerous, and lived a considerable time with me, I was never able to discover their mode of increase. They were never seen double—"two single gentleman rolled into one"—as the convivial Vagincola appears to be when undergoing multiplication. Two Lagotias, indeed, keeping house in the same bottle would doubtless lead a most unhappy life. The single tenant is an ill-conditioned and restless fellow, constantly rotating this way and that, and wagging his long ears; and, when sitting for his portrait, assuming as many changes of character as Charles Matthews himself.

The colour of the body of *L. viridis* is not caused by an accumulation of green granules as in *Stentor Ophrydium* and *Vorticella*, but is a transparent and uniform staining of the sarcode—a lighter tint of that of the cell.

In young specimens found growing amongst the second batch, the lobes of the horse-shoe were blunt and short, and the ciliary band placed at a little distance from their edges, as in fig. 4.

L. hyalina.—Colourless; lobes of rotatory organ wider and blunter than those of *L. viridis*. Cell buried in the substance of *Alcyonidium hirsutum*, and therefore not seen. Found at low-water, Granton and Queensferry. Not uncommon.

L. atro-purpureus.—Colour of animal that of a mixture of ink and water. Cell yellowish-brown. Probably a variety in colour of *L. viridis* with which it was found.

[Since the above was communicated to the Royal Physical Society, I have learnt from Mr Alder that he has occasionally seen *L. viridis*, and he has sent me drawings of specimens obtained in autumn last near Tynemouth. In these the spiral gullet does not appear. Mr Alder thinks that the animalcule sometimes burrows in the shells which it infests, as I have noticed in the case of *L. hyalina*.

At fig. 5 I have given a sketch of *Vaginoola ampulla* (Müller), which has a bilobed rotatory organ, and so far bears some resemblance to Lagotia.]

Vagincola valvata. (Mihi). (Plate VI., figs. 6, 7).

This marine animalcule was found growing plentifully on the zoophytes and sea-weeds in one of my tanks. It resembled *Vagincola crystallina*, an inhabitant of fresh water, except in its being colourless, whilst *V. crystallina* contains globules of green matter. It possesses another remarkable distinction also from *V. crystallina*, in the presence of a valve (*a*) situated within its cell, which shuts down in an inclined position (*b*) over the animal as it retreats therein. On examining the valve *in situ* I found it to consist of a rigid plate, imbedded in a thick layer of transparent sarcode, which latter was continuous at the lower end of the valve with a thin layer of the same substance, lining the whole of the interior, and coating the upper part of the exterior of the tube. The valve was closed by a contractile process passing from its under surface (fig. 8, *c*.) to the wall of the tube. I have not been able to come to any conclusion as to the shape of the solid frame-work of this remarkable provision for closing the cell of this animalcule, as it is visible only in profile; but I am disposed to consider the whole apparatus to consist of an oval plate of soft sarcode, supported by an included bar or narrow plate of horn or chitine. It is evident that a rigid oval plate accurately closing the bore of the tube would be immoveable.

The animal was generally double, as in the figures. In some specimens the tube was marked with close transverse or circular striæ.

*Ephelota coronata.** (Mihi).

In the seventh volume of the "Annals of Natural History" (1851), Mr Alder has described three new animals, belonging to the Protozoa, two of which are marine, and found parasitic on Sertularia, while the third is an inhabitant of fresh water, and a parasite on Paludicella. Mr Alder gave no names to these animals. It therefore fell to Mr Pritchard (who, in his work on the Infusoria, has included them in the family Enchelia) to invent a name for them. Mr Pritchard chose the designation *Alderia*, and specified the animals as *apicu-*

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* Communicated to the Royal Physical Society, Nov. 25, 1857.

losa, *ovata*, and *pyriformis*. "Alderia" had, however, been previously appropriated to one of the nudibranchiate Mollusca, so that the animals still remain without generic names. On carefully reading Mr Alder's descriptions, and comparing them with the descriptions and figures given by Ehrenberg of *Podophrya fixa* and *Acineta Lyngbyei*, I have concluded that Mr Alder's animals should be placed in two genera; that Pritchard's two species, *ovata* and *pyriformis* (the tentacles of which are slender and capitate, or knobbed) belong, together with *Acineta Lyngbyei*, to the genus Podophrya; whilst *apiculosa* (the tentacles of which are pointed) must be referred to a new genus, for which I propose the name Ephelota (from ἐπι and ἧλος, a peg, and its derivative adjective ἡλωτος).

The body of *Ephelota apiculosa* (Alder's first described animal) is cup-shaped, set round with numerous pointed tentacles abruptly thickened towards the base, and forming more than one row. They have very little motion, but are occasionally bent forwards, and sometimes slowly retracted. Body attached to a stout stem. In Mr Alder's figure the stem appears of the same thickness throughout. I have occasionally found an animal, which I believe to be identical with *Ephelota apiculosa*, growing on Coryne. It differs from *Ephelota coronata* (the animal I have figured, Plate VI., fig. 1), in having the body more cup-shaped, elongated, and wider than the stem; the tentacles more irregular, soft, retractile, and unsupported by the solid matter which occurs in the interior of those of *Ephelota coronata*; and, especially, in the shape and structure of the stem, which is nearly of equal diameter throughout, and consists of a *medullary substance*, the fibres of which pass in a longitudinal direction, inclosed within a *cortical substance*, formed of circular fibres, passing at right angles to the fibres of the medulla—which cortical fibres are absent in the stem of *Ephelota coronata*.

I have found *Ephelota coronata* only twice, each time in large colonies, situated within the mouth of shells inhabited by the hermit crab, where the dense white bodies of the animalcules, seated on their transparent pedicles, form sufficiently remarkable objects.

The body consists of a short cylinder of densely granular sarcode, slightly enlarged above and below, so as to resemble the circlet of a crown. It is surmounted by a circle of thick, acuminate, and radiating tentacles, which are capable of being slowly curved inward, but cannot be contracted. They remain stiffly extended, even when the animal is immersed in alcohol. The structure of the tentacles is, I believe, unique. Under high microscopic power, they are seen to consist of a bundle or frame-work of fine parallel rods of horny (?) texture, embedded in soft contractile sarcode. The more central rods of the bundle (as in the figure 2) protrude continually beyond those exterior to them, so that the point of the tentacle is formed of only a very small number. In the animals of the second colony—under a power of 800 diameters—each rod assumed a beaded structure (fig. 2), which I had not before observed.

The animal secretes beneath itself, or from its base, a pedicle of diaphanous and colourless substance, which increases in length and breadth with the increasing growth of the animal, until it assumes the form of a long glassy club, on the thick upper extremity of which the animal is seated. The whole of the pedicle is covered by a growth of scattered hairs, but it may be doubted whether these have any organic connection with it, and whether they do not belong to one of those minute classes of Algæ, the structure of which eludes microscopic research. A longitudinal fibrous structure is faintly seen in the axis of the pedicle, but it gradually disappears towards the periphery. After immersion in spirit, this fibrous structure becomes much more apparent. The action of the spirit, also, causes a fine membrane to separate from the surface of the pedicle, which appears to be continued downwards from the body of the animal, and is probably analogous to the membrane which I have already shown to exist as a lining and covering to the cell of *Vagincola valvata,* and which secretes and hides within itself the valve that closes the cell of that curious animal.

Observations on British Zoophytes. By THOMAS STRETHILL WRIGHT, M.D., &c.

EXPLANATION OF PLATE.

Fig. 3. Medusoid of *Campanularia Johnstoni*—*a* ovaries.
4. Ovary of do., with ova.
5. *Coryne gravata*, with medusoids—*a* peduncle=sperm-sac—*b* polyp undergoing absorption.
6. *Stauridia* of Dujardin (after Gosse).
7. *Stauridia producta* single polyp.
8. End of one of the capitate tentacles of *S. producta*—*a* head covered with thick prehensile palpocils, and containing thread-cells—*b* ectoderm, with acuminate palpocils springing from tactile (?) corpuscles—*c* central chain of endodermal cells, with vacuolated contents, nucleus, and brown granules.
9. Thread-cell of *S. producta*, with thread exserted.

*Coryne gravata.** (Mihi). (Plate VII., fig. 5.)

In the spring of 1856 I noticed, in a rock-pool near North Berwick, a number of small milk-white bodies, apparently floating in irregular lines, at about half an inch from the surface of the friable sandstone. When these were transferred to the collecting-bottle, they were seen to be attached to the buccal papillæ of small coryneform polyps, and proved to be the greatly enlarged peduncles of fully-developed medusoids.

The polyps of the Coryne were colourless, with ten or twelve short capitate tentacles; the polyp-stalks smooth, about a quarter of an inch long, springing from a creeping polypary; the medusoids colourless, long, cylindrical, with four lateral canals and four rudimentary tentacles, represented by small bulbs containing brown pigment; the peduncle, pyriform, inflated,—nearly filling the cavity of the sub-umbrella; the umbrella without thread-cells, wrinkled on its external surface.

Further observation showed that the peduncle of the medusoid, though still attached to the Coryne by a thick fleshy process, had become little else than a sac of spermatozoa, which were secreted between its ectoderm and endoderm. These membranes were continuous with each other at the mouth, where they were furnished with a ring of thread-cells.

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* Communicated to the Royal Physical Society of Edinburgh, on the 25th April 1857.

In several cases the bodies of the Corynes had lost their tentacles, and were reduced by absorption to mere tubercles (fig. 5, *a*), the medusoids still remaining firmly attached; hence it is possible that the medusoids of this Coryne never become free.

In the "Annales des Sciences Naturelles," vol. xv., 2d series, Lowen has described a Coryne (*Syncoryne ramosa*) bearing a fixed medusoid, strongly resembling the above, with the exception that the peduncle contained ova instead of spermatozoa. I was at first led to believe that my Coryne was the *male* polypary of Lowen's *female*; but the wrinkled corallum, or polypidom of the latter, and the presence of thread-cells on the umbrella of its medusoid, indicate that the species, although similar, are distinct.

The "*Syncoryne ramosa* (Ehr.)" of Lowen, differs, I think, altogether from the *Coryne ramosa* described by Johnston as "bipollicaris, hyalina, ramosa, ramulis basi contractis, capitulis valdè elongatis, prole in capitulo sparsa, (Ehr.)" (a large branched Coryne with a ringed corallum, found in the Firth of Forth, and remarkable for the length of its cylindrical polyps, and the number of ovisacs or sperm-sacs scattered amongst its tentacles), and will have to be referred to a new species.

For the subject of this notice I propose the name of *Coryne gravata*.

The Rev. T. Hincks informs me that he has seen a Coryne with cylindrical medusoids resembling fig. 5, but he did not observe its sexual character.

Stauridia producta (Mihi).

In the "Annales des Sciences Naturelles," 3d series, vol. iv., p. 271, M. Dujardin remarks, "J'y vis une sorte de Syncoryne que j'ai nommée Stauridie à cause de ses quatre tentacules disposés en croix;" and he proceeds to describe the structure of the animal, and its reproduction by means of free medusoids, to which he gives the name of *Cladoneme*.

Mr Gosse, also, in his "Devonshire Coast," has described and beautifully figured the same animal, under the name of *Coryne stauridia*, or "the slender Coryne." This zoophyte, one of the polyps of which I have sketched at Pl. VII., fig. 6., is remarkable, not so much on account of the cruciform disposition of its tentacles, as for the dissimilarity in character of those members; the upper row being capitate, as in the genus Coryne, while the lower are filiform and pointed, as in Clava, Cordylophora, &c.

The dissimilar character of the tentacles, however, must remove the animal of Dujardin from the genus Coryne or Syncoryne, and place it in a new genus, which will rank intermediately between Coryne and Cordylophora in the classification of Johnston. For this genus I propose the name "Stauridia," derived from Dujardin's Stauridie, although the construction of the word and its meaning are imperfect (σταυρος, *crux*, signifying a stake, *of any shape*, to which a criminal was nailed.)

The characters of the new genus are:—

Stauridia.

Polypary sheathed in a tubular corallum or polypidom (branched, the apices of the branches) bearing polyps furnished with two or more whorls of dissimilar tentacles,—the upper whorl or whorls capitate, the lower whorl filiform, four in number. Thread-cells very large, many-barbed.

In the spring of 1857 I picked up, on the shore at Caroline Park, near Edinburgh, a specimen of *Plumularia falcata*, on which grew a coryneform zoophyte belonging to the genus Stauridia. The polyps were very long and cylindrical, and furnished with twelve capitate tentacles, arranged in three whorls, and also with a fourth whorl of filiform tentacles, situated at a considerable interval beneath the third whorl, as shown in fig. 7. The filiform tentacles were held, not at right angles as in Dujardin's species, but at an acute angle with the body of the polyp.

The globular tips of the upper tentacles exceeded in size those of any of the Corynes with which I am acquainted, and contained many-barbed thread-cells (fig. 9), half a diameter larger than similar cells in *C. pusilla.* When first found, the smooth polyp-stems sprung singly from a creeping fibre; but after a few weeks of plentiful entomostracean diet, they became, irregularly branched, as in Gosse's figure of *Coryne stauridia.*

I have named this species *Stauridia producta.*

S. producta.—Polyps much elongated, cylindrical (reddish); capitate tentacles in two or three whorls; filiform tentacles semi-erect.

Dujardin has described, in his Stauridie, the production of medusoids whose strange form and precautions for the safety of their ova are well worthy of note. I anxiously watched my Stauridia for many weeks, but it gradually died away, "without issue."

The tentacles of Coryne and Stauridia are not hollow, but contain a core or central chain of endodermic cells, placed in single series (as at *c*, fig. 8). The contents of each of these cells consist of highly vacuolated sarcode, which includes a nucleus, accompanied by a few coloured granules, the function of which has not been determined. The ectoderm of the tentacle *b* is not generally vacuolated; it contains minute soft corpuscles (woodcut, fig. 2), from each of which projects externally a long and finely-acuminated spine or palpocil. These spines are also found scattered over the whole body of the polyp, unconnected with thread-cells, and are, I am led to believe, instruments of sense (touch?). The head of the tentacle *a* is covered with short thick palpocils, which I have elsewhere considered as prehensile apparatus. These palpocils (woodcut, fig. 1) arise each as a somewhat rigid process, from the side of one of the large thread-cells buried in the head of the tentacle, and they probably convey an impression from bodies coming into contact with them, to the thread-cell, which causes the extrusion of its dart. The offensive character of the thread-cell, which has been denied, (Lewis' "Sea-side Studies"), is proved by allowing any

Fig. 1. Fig. 2

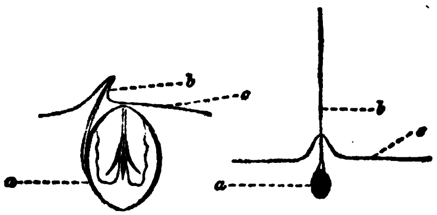

Fig. 1.—*a* thread-cell—*b* thick palpocil—*c* surface of head of tentacle.
Fig. 2.—*a* tactile corpuscle—*b* acuminate palpocil—*c* surface of ectoderm.

soft body, as the bulb of a human hair, to touch the tentacles of Hydra, Coryne, or Actinia, when it will afterwards be found studded with their darts buried beyond the barbs.

Reproduction by ova of the Medusoid of Campanularia Johnstoni.

Campanularia Johnstoni occurs commonly at low-water, on the shores of the Firth of Forth, and is doubtless very familiar to many of the members of this Society.

At all seasons of the year it produces campanulate medusoids (Plate VII., fig. 3), each having four tentacles capable of great extension, four intermediate rudimentary tentacles and eight auditory organs, consisting of sacs, each containing a single spherical crystal of carbonate of lime, and situated one on each side of each of the rudimentary tentacles.

The medusoids are produced within coarsely-annulated capsules, developed generally from the creeping fibre which unites the polyp-stems; but sometimes also from the polyp-stem itself, in which case the stem is generally branched, and the capsule axillary. The capsule is traversed by a fleshy axis, dilated at its summit. From this axis, which may be considered as a reproductive polyp, homologous with the reproductive polyp of Hydractinia, the medusæ pullulate, inclosed within sacs formed by a layer of ectoderm derived from the fleshy axis of the capsule. The tissues of the medusoid are developed from, and continuous with both the layers (endoderm and ectoderm) of the axis, and are at first a mere diverticulum thereof, as I have described to the Society in the case of the medusoid of Eudendrium.

The medusoid of *C. Johnstoni* was described by the Rev. T. Hincks in August 1852, and again by Mr Gosse in 1853, neither of which gentlemen detected in it any ovaries, though Mr Gosse has figured enlargements on the lateral canals (fig. 3, *a*), where those organs exist.

In spring last, I obtained a large number of these medusoids from a specimen of *Campanularia Johnstoni* in my possession; and on examining some of them directly after their escape from the capsule, I was surprised to find that the enlargements figured by Gosse contained ova (fig. 4), with the germinal vesicle clearly distinct, under a power of 600 diameters.

I placed a number of the medusoids in a flat and deep glass saucer of pure sea-water, and in about a week young Campanularias were found attached to the bottom of the saucer, each of which consisted of two polyp-stalks, united by a creeping fibre.

The various stages of development in the ova were not observed, on account of their extreme minuteness.

In confirmation of these observations, Mr Alder writes me, in answer to my statement of the fact above mentioned to him, that Mr Hincks made similar observations at the Isle of Man, in autumn last. And further, my friend Mr Dallas, Edinburgh, informs me, that he also observed a number of young produced in a vessel containing *Campanularia Johnstoni* in spring last.—[*Read, Nov.* 25, 1857.]

On the Density of Bromine Water of various Strengths. By J. Slessor, Assistant to Professor Anderson, Glasgow University.

From the frequent use of bromine water as a reagent in chemical inquiries, it is often desirable to know its strength; and it occurred to me, that the specific gravity might afford an indication of the quantity of bromine in solution, which could be rapidly obtained, and be near enough for many purposes.

Accordingly, I have made a number of determinations of the specific gravity of bromine water, of various strengths, determining the bromine by two processes, first, as bromide of silver; and secondly, by the process proposed by C. Greville Williams, by decolorizing with turpentine. The latter process I have found easy of performance, and very convenient, where a number of experiments are required; one standard solution serving for all. It is founded on the fact, that when turpentine is added to a solution of free bromine, it forms a colourless compound with it; 80 parts of bromine (1 atom) requiring 34 parts turpentine (C_5H_4) for its complete decolorization. To perform the process, a standard solution of turpentine in spirit of wine is gradually added to the bromine water diluted in a stoppered bottle, with agitation. The entire absence of colour shows when enough turpentine has been added. The strength of the turpentine solution used in these experiments was one grain turpentine, to the cubic inch of spirit of wine, except in the stronger solutions of bromine, when the turpentine was doubled. A burette, containing a cubic inch divided into 100 parts, was employed.

The following are the results obtained in trying the accuracy of the process :—

I. 173·475 grains bromine water used in each experiment.

1st. 78·5 measures used = 1·84 grains bromine.
2d. 79· ,, ,, = 1·86 ,, ,,
3d. 79· ,, ,, = 1·86 ,, ,,

Bromide of silver obtained :—

4th. 4·40 grains = 1·87 ,, ,,
5th. 4·42 ,, ,, = 1·88 ,, ,,

II. Another preparation, 251·12 grains used in each experiment.

1st. 42·5 measures used = 1·00 grain bromine.
2d. 43· ,, ,, = 1·01 ,, ,,

Bromide of silver obtained :—

3d. 2·335 ,, ,, = ·99 ,, ,,
4th. 2·39 ,, ,, = 1·02 ,, ,,

These experiments being quite sufficient to show the accuracy obtained by this process, the specific gravity of the bromine water was now taken at 60° F. The following table shows the numbers obtained :—

Specific Gravity.	Bromine in 1000 grains Bromine Water. Turpentine determinations.	Bromine in 1000 grains Bromine Water, by Bromide of Silver.
1003·95	3·98	3·94
	4·02	4·05
1005·69	6·29	
	6·29	
1006·08	7·11	
	6·94	

Specific Gravity.	Bromine in 1000 grains Bromine Water. Turpentine determinations.	Bromine in 1000 grains Bromine Water, by Bromide of Silver.
1009·01	10·72	10·78
	10·68	10·83
1009·31	12·05	
	12·31	
1009·95		12·82
		12·92
1012·23	14·97	
	15·04	
1014·91	18·74	
	19·06	
1015·85	19·52	
	19·88	
	20·09	
1018·07	21·22	
	21·55	
	20·89	
Saturated.	31·02	31·31
1023·67	30·86	31·69
		31·10

These results show that 1000 parts of water dissolve 4 parts of bromine without sensibly altering in volume; but above this the specific gravity does not keep pace with the addition of the bromine, the difference being greater as the solution becomes stronger; and when we reach saturated bromine water the specific gravity is 8·0 lower than the bromine in solution.

The quantity of bromine in saturated bromine water, according to Löwig, who states that 1 part bromine dissolves in 33 parts water at 60° F., is 29·16 grains in 1000 grains of the bromine water. The numbers I have obtained gave 31 parts as the quantity of water required to dissolve 1 part of bromine.

These experiments were made in the laboratory of Dr Anderson, to whom I return my best thanks for allowing me an opportunity to perform them.

EXTRACT FROM CORRESPONDENCE.

Letter from G. GARCIA MORENO *to* Professor W. JAMESON, *relative to the exploration of the Volcano of Pichincha.*

QUITO, 13*th Jan.* 1858.

MY DEAR FRIEND,—The following is a brief account of my last exploring expedition to the volcano which overlooks Quito. The short distance at which the volcano Rucu-Pichincha is situate from this city, has contributed to excite the curiosity of the scientific travellers who have visited the territory of the Ecuador, and have caused the state and form of the volcano to be well known. Bouguer and La Condamine, in 1742, were the first who reached the brink of the crater: the celebrated Alexander Humboldt, in May 1802, twice surmounted the gigantic wall of Dolerite which forms the eastern border of the volcano; and, about thirty years after, your countryman, the unfortunate Colonel Hall, and M. Boussingault, followed in the same path; but since 1844, in which M. Sebastian Wisse and I descended to explore it, no one had reached the bottom. In August 1845 we returned with the intention of making the topographical plan of the volcano, measuring heights, &c.; and in order that we might do this, we had to pass three days and three nights in the two deepest cavities which form Rucu-Pichincha.

In an orographical view, our second expedition gave us the results which we longed for. Rucu-Pichincha, placed to the S.W. of Quito, forms two great basins, one to the east of the other, 4921 English feet in length. The eastern basin, called, without sufficient reason, "Eastern Crater," has the form of a narrow valley, long and deep, through the middle of which passes from north to south a fissure which receives the rain and melted snow: there exists a slight depression in the upper part of this basin of an elliptical form, and perfectly horizontal at the bottom, similar in everything to a little alpine lake dried up by the action of the sun: a depression which at one time, from its form, gave rise to the belief in the existence of an inactive crater. The depth of this supposed crater is 1050 feet below the wall of eastern rocks; and as the highest of these reaches to 15,748 feet above the level of the sea, the height of the bottom of the eastern crater is 14,875 feet.

The western basin, or, more properly, the true crater of Pichincha, is one of the most imposing objects which is presented to naturalists—placed on the western slope of Rucu-Pichincha, differing from the other craters of the Ecuador, which take the form at the summit of a regular cone covered with snow, it presents the figure of a truncated cone placed upon its inferior base, which is 1476 feet in diameter, and rises in height to 2296 feet. Its depth from the eastern side is enormous; and when one gazes upon the immense towers of dolerite and trachite elevated 2460 feet, sometimes vertically, sometimes in slopes more or less steep and varied, one receives an impression which is never effaced through life. Towards the western part the height of the walls of the crater diminishes gradually, leaving open to the east a fissure from whence the united waters escape during the rains or thaws.

In the middle of the inclined plain which constitutes the bottom of the volcano, the actual cone of eruption rises; it is 820 feet in diameter, 262 in height above the bottom of the middle of the crater, and 13,707 above the level of the sea, standing 4166 feet above Quito. This little mountain is now the centre of volcanic activity in Pichincha, and presented in 1845 clear indications of remaining permanent many years without increase of intensity. A great part of this mountain is covered with vegetation; two regions, parting in opposite directions, completely gird it, until they are united in the cleft of which I have spoken; and in the two points from whence the cone of eruption is depressed (one to the centre, the other to the S. E.) there is given out in abundance a hot and sulphureous vapour which lines with sulphur the holes and interstices between the fragments of rock of which the cone is composed.

We failed, in the expedition of 1845, to study the volcanic and vegetable products which the crater presented. In order to examine its actual state, and to fill this blank, I descended on the 16th of last December, carrying, as far as possible, what was necessary for the perilous situations in which I expected to be placed. I was engaged little more than three hours in the descent; and half-past eleven of the day found me at the cone of eruption. The form which this presents, proves that the bottom of Pichincha has been recently the theatre of considerable convulsions. The vegetation which covered it has disappeared from the eastern side; the depression which exists towards the south-east, at the foot of the cone, has widened itself, and has filled up a part of the broken inclosure, interrupting it perpendicularly with a broad wall of stones, undoubtedly shot out from its interior. Near to this, and towards the south, it has formed, since 1845, a new depression, or, speaking more properly, a new accidental crater, from whence arises a great mass of vapour, so that the cone of eruption has at present three apertures or craters: the principal occupying the higher part; the ancient accidental crater, placed at the south-east, and at the foot of the former; and the new accidental crater, open likewise at the foot and at the south of the principal one.

The volcanic activity of Pichincha has increased remarkably, as is manifested by the greater exhalation of vapours. In 1845, the chimneys from whence the gases arose formed six groups, of which only one was considerable. Now the vapours escape by innumerable interstices and hollows, which the stones leave in each of the craters; and in the principal one is heard a noise resembling that made by an immense cauldron of boiling water.

The temperature of the vapours varies much in the different interstices. In the crater of the south-east the vapours of the highest interstices are nearly 188°·6 Fahr., whilst in the lower ones the temperature was only 140° Fahr. In the principal crater the hottest vapours did not come up to 194° Fahr.; in the largest interstice that I have observed, into which a person could easily enter, if the thick column of vapour would permit him, the temperature was only 98·6° Fahr. at three feet of depth. Filling a graduated tube with water, and placing it within the interstices, I collected the gases several times in order to analyze them, and, moreover, condensed them by means of a bottle filled with cold water, and gathered the drops of fluid which were formed. The result of my observations is, that the gases of Pichincha contain a scarcely perceptible trace of sulphurous, sulphuric, and sulphyhydric acid, four per cent. of carbonic acid, and the rest composed exclusively of water. I present these results only as approximate ones. The atmospheric air is always mixed with the volcanic gases in those points where it is possible to collect it; and this cause of error is inevitable, without reckoning those which occur from the personal difficulties of the observer.

The solid products of the volcano are the sublimed sulphur which covers almost all the stones and fissures; and a white salt which appears in silky fibres, and shows itself in many of the interstices, sometimes alternating with the flower of sulphur in parallel coatings, sometimes in an abundant and pure mass. This salt is a double sulphate of alum and of the protoxide of iron, likewise formed in other volcanoes, and known by the name of "*alumbre de pluma*," or plumose alum. Dissolved in water, it crystallizes by spontaneous evaporation in a derivative form of the oblique rhomboidal prism. Besides these products, there is found scoriæ, composed of melted sulphur and ashes of pyroxene and dolerite, more or less calcined or altered by the action of the watery vapours.

The plants which I collected in the crater, and which you had the goodness to name, are:—*Alchemilla nivalis*, *Ranunculus Gusmanni*, *Jamesonia* sp. (these two plants were found nowhere else but on the ridge of Pichincha); *Culcitium reflexum*, *Werneria graminifolia*; *Gaultheria myrsinoides* (the space of ground in which this little shrub grew showed a high degree of temperature—87° Fahr.); *Polypodium crenulatum*, *Porerretia pyramidata*.

I came out of Pichincha on the 17th of December, after having passed the previous night within the crater, and at 493 feet from the cone of eruption. Desirous of continuing my observations, I retain the hope of returning to the crater in the present year, in order to pass some days within it, and I will only consider my late expedition as a preparatory and necessary step towards another more important one. Before undertaking it, I will ascertain the point from whence the descent to the bottom of Pichincha is likely to be easiest, avoiding the imminent peril of precipitating oneself when descending the eastern wall. In 1844 M. Wisse fortunately saved himself in time from rolling headlong into a horrible abyss. A similar accident befell me in 1845; and in December last year, your son, who accompanied me, nearly found his grave in the abyss. I have no doubt that in descending 2460 feet of rocks, in which hands are more useful than feet, a single rash step would be followed by fatal consequences.—I am, &c., G. GARCIA MORENO.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 7th December 1857.—At the request of the Council, Dr **Christison**, V.P., delivered an opening address from the Chair, The following communication was read:—

Excursions in the Troad, with observations on its Topography and Antiquities. Part I. By Dr **William Robertson**, F.R.C.P. Communicated by Dr J. Y. **Simpson**.

Monday, 21st December 1857.—Professor **Kelland**, V.P., read from the Chair Biographical Notices of MM. Thénard and Cauchy, two recently-deceased foreign Members of the Society. The following communications were read:—

1. *Excursions in the Troad, with Observations on its Topography and Antiquities.* Part I. By Dr **William Robertson**, F.R.C.P.E. Communicated by Dr J. Y. **Simpson**.

The author had resided for fifteen months, in 1855–56, within a few miles of the Plain of Troy, and had made excursions over it at all seasons.

His paper commenced with a description of the western extremity of the Asiatic coast of the Hellespont, between Abydos and Koum-Kaleh, including the River Rhodius and the sites of Dardanus, Ophrynium, Pteleos, Rhætium, and Novum Ilium, all of which he considered positively identified. A minute topographical account followed: first, of the valley of the Dumbrek (Simois); next, of the valley of the Mendere (Scamander); next, of the valley of the Kimair (Thymbrius of Strabo); and lastly, of the hilly country between these streams, and of the relics of antiquity which it included.

The author believed that Homer's Troy must have stood, like the Novum Ilium of Strabo, on the hill now called Hissarlik—that the mouth of the Scamander was formerly two miles to the east of its present main channel, and that the In-Tepeh-Osmak and Kali-fatli-Osmak might be regarded as its terminations in the times of Homer and Strabo. He showed that these, and the other Osmaks in the valley of the Mendere, were at present merely winter channels of the river, and that in summer they would be dry nullahs, but for the drainings from the extensive marshes left by the winter inundations of the plain. He believed that the bay between Koum-Kaleh and In-Tepeh was deeper in the days of Homer, and that its eastern extremity, in particular, had, during the last 2000 years, been materially encroached upon by deposits of mud and sand from the rivers and sea.

He remarked that Homer made no mention of a *river* Thymbrius, and that the Thymbra which is alluded to in the Iliad very probably stood in the valley of the Simois, to which it has ultimately transferred its name. The Thymbra and Thymbrius of Strabo were certainly situated near the modern farm of Ak-tchai-kioi on the Kimair.

An account was given of various excavations, made in 1856, in some very ancient places of sepulture at Ak-tchai-kioi, and among the ruins of Dardanus. In the former of these cemeteries the bodies had been buried, entire and unburnt, in very large earthen urns, along with pateræ and lachrymatories, of materials and forms indicating the earliest stage of Grecian art. The cemetery at Dardanus was more modern; and the bodies, which had usually been burnt, were here found in rectangular cysts, built of flat stones or tiles, and carefully cemented. The pottery found at Dardanus was often of very elegant workmanship, and the painted or glazed figures upon it less rude than those observed at Ak-tchai-kioi; but it was singular that no medals, nor coins, nor even traces of inscription, had been found among these tombs.

2. *On the Composition of the Building Sandstones of Craigleith, Binnie, Gifnock, and Partick Bridge.* By Thomas Bloxam, Assistant Chemist, Laboratory of Industrial Museum. With a Preliminary Note by Professor George Wilson, Director of the Industrial Museum.

(This paper appeared in last No. of this Journal, p. 83.)

Monday, 4th January 1858.—The Right Rev. Bishop Terrot, V.P., in the Chair. The following Communications were read:—

1. *On the Structure of the Reproductive Organs in certain Hydroid Polypes.* By Dr Allman.

Most of the observations contained in the present communication were made some years ago; and during the last autumn the author had an opportunity of repeating many of them, and of adding some others. His object in now bringing them together was, that, by being thus placed in possession of sufficient material, we might be enabled to make a useful correlation of the ascertained facts, so as to obtain, if possible, some more general expressions for the phenomena presented.

To arrive at such results, it was found absolutely necessary to introduce some new terms; for, in many cases, parts requiring precise notions had no distinctive appellation whatever, while in other cases they had been known by names which convey an entirely false idea of their nature and significance.

Definition of Terms.

The parts of the hydroid zoophytes, on which devolve the office of perpetuating the species by the exercise of a true generative function as distinct from simple gemmation, show themselves, as is well known, under the condition of external buds, which are produced in various forms and and in various positions on the animal. To these buds, which are truly sexual, being in some cases male and in some female, the author proposes to give the name of *gonophore* (γονος, φορεω).

As an essential portion of the gonophore, we invariably find one or the other of two different kinds of bodies. One of these presents the form of a closed sac, in which a more or less disguised medusoid structure may, in almost every instance, be detected. For these bodies he proposes the name of *sporosacs* (σπορα σακκος).

The other differs in no respect from a gymnophthalmous *medusa*, and may conveniently be designated by this name.

Both sporosacs and medusæ contain the immediate products of the generative system, certain individuals of each producing ova, and certain others spermatozoa.

In some cases the gonophore has only a single sporosac, or a single medusa, and these spring directly from the cœnosarc of the zoophyte. In other cases these bodies are numerous, or, if single, do not spring directly from the cœnosarc of the zoophyte, but from a special organ (blastostyle) to be presently described. In the former case, the gonophore may be called *simple* (*Gonophora simplex*); in the latter, *compound* (*Gonophora composita*).

The simple gonophore consists essentially of a sac, which is a mere extension of the ectoderm of the zoophyte invested or not invested by a polypary, and containing within it, in some cases, a sporosac, in others, a medusa. To this sac the author gives the name of *ectotheque* (*εκτὸς, θηκη*).

A correct notion of a compound gonophore may be best obtained by referring to some illustrative example, such as that afforded by a Laomedea.

In this genus the gonophores are produced near the axils of the ramuli in the form of oval hollow bodies or capsules, invested like the stems and ramuli by a distinct polypary. The axis of the capsule is traversed by an extension of the cœnosarc of the branch in the form of a tubular column, from whose sides there bud forth in some cases numerous sporosacs, in other cases medusæ, each with an ectothecal investment from the column. For this column the author proposes the name of *blastostyle* (*βλάστη, στύλος*).

In some cases (e.g., *Tubularia*) the compound gonophore is destitute of its investing capsule, and then presents merely the condition of a naked blastostyle with its sporosacs (or medusæ ?).

The medusæ, like the gymnophthalmæ, generally consist of an *umbrella* or *mantle* with radiating and circular canals, and with a central projecting organ, in which the stomach is excavated, and which carries the mouth at its extremity. To this central organ the term *peduncle* has commonly been given, a name which conveys a wrong idea as suggestive of an organ of attachment and support. It is also frequently called the stomach, a term also obviously incorrect, as the true stomach may really occupy but a small portion of the entire organ. Huxley,* seizing, as the author thinks, upon its true significance, names it *polype;* but as it will be more convenient in the present investigations to distinguish it from the ordinary nutritive polype of the colony, it is proposed to speak of it under the name of *manubrium*, a term suggested by its position with regard to the mantle being such as to admit of a comparison with that of the *handle* of an umbrella.

The sporosacs consist of parts which have their strict homologues in the medusæ. These parts will therefore be spoken of under the same names as those of their equivalents in the medusæ.

These preliminary remarks will render easily intelligible the new terms which, after much consideration, the author deemed it necessary to introduce, as the only way by which cumbrous circumlocution can be avoided and precision given to our descriptions; and he proceeded in the next place to describe the structure of the reproductive system in certain species which have in this respect either never received, so far as he is aware, the attention of the comparative anatomist, or which, though studied to a certain extent, still present certain points worthy of attention, but which have hitherto escaped notice.

Hydractinia echinata.

In *Hydractinia echinata* the gonophores are borne upon certain polypes, which, as is well known, are destitute of tentacles and mouth, and differ also in some other respects from the other digestive polypes of the colony.† The gonophores surround the naked stems of the polypes at a short distance behind the distal extremity. They may be seen to consist externally of an investing sac (ectotheque), which is a simple extension of the ectoderm of the polype, like it containing thread-cells, and totally destitute of polypary.

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* Lectures on General Natural History in *Medical Times*.

† I have observed in these generative polypes an oval mass nearly filling the cavity of the body. It is developed from the endoderm, and projects from the floor of the cavity. It reminds one of the manubrium of a sporocyst, but is apparently solid.

Immediately within this is another sac (sporosac), in which the indications of decided structure are very obscure. The second sac immediately invests the mass of ova or spermatozoa which occupy the space between its wall and a well-developed manubrium, which lies in the axis of the sac. The manubrium is a simple diverticulum of the endoderm of the polype, its cavity freely communicating with that of the latter. I could find no evidence of an ectodermal layer upon it. There are no gastro-vascular canals.

I have not succeeded in making out any further structure in the gonophore of Hydractinia which may be assumed as a type of the simple gonophore.

Hydractinia echinata is strictly diœcious, the male and female gonophores being always separated, so as to occupy distinct colonies.

Coryne ramosa.

The gonophores here belong to the simple type. They are borne upon the clavate body of the polypes, where they are scattered irregularly among the tentacula.

They are of a nearly spherical figure, and are attached to the polype by a short peduncle.

The manubrium of the sac is large and simple,—there are no radiating canals.

In the female gonophores the ova are numerous, and may be seen in their young state to be each contained in a very delicate membraneous cæcal tube of a pyriform shape, which closely embraces the ovum, and is attached by its narrow extremity, which constitutes a sort of neck to the base of the manubrium. The germinal vesicle and germinal spot are distinct. As the ova advance towards maturity, they appear to rupture the confining membraneous tube, and then lie free in the cavity of the gonophore.

In the male gonophores, while they cannot be externally distinguished from the female, the sporosac is filled with the spermatogenous cells. These may be seen, under slight compression, to be arranged in radiating lines, which, with a little careful examination, may be traced to the base of the manubrium. That these lines represent exceedingly delicate tubules, filled with the spermatogenous cells, seems evident, and then they will be the exact equivalent of the pyriform ovigerous tubes of the female. It is quite possible that, in both the male and female sacs, the tubes containing the spermatozoa in the one, and the ova the other, open into the cavity of the manubrium; and thus facility would be at once afforded for the spermatozoa to gain access to the ova. I have never, however, succeeded in demonstrating such a communication.

Indications of the ova and spermatogenous cells being confined at an early period within delicate membraneous tubes may be witnessed in other species, but in no case have I succeeded in demonstrating such a condition so plainly as in the present species.

The spermatozoa have the usual form of caudate corpuscles. The zoophyte diœcious.

Clava multicornis.

In this species, the gonophores are borne upon the clavate body of the polype, just where it passes into the stem, and immediately behind the posterior tentacula. They are compound, each consisting of a cluster of sporosacs attached to a short blastostyle, but are destitute of investing capsule. I have observed in the same colony two kinds of polypes,—the ordinary tentaculiferous polypes, and others destitute of tentacula, and consisting merely of a columnar stem, scarcely clavate at its extremity, and destitute of mouth. The gonophores were borne on both kinds of polypes. The male and female gonophores are separate on distinct colonies.

The manubrium of the sporosac is simple; and there are no radiating canals. In each sporosac (female) there is usually a single ovum, though I have occasionally witnessed two. The germinal vesicle is visible in the ovum, and the process of segmentation may be distinctly traced.

As development proceeds the ovum becomes elongated, and it may be seen to be invested by a proper membrane, apparently structureless. Within this membrane a distinct dermal layer now begins to be differentiated from the ovum; while at the same time a cavity is formed within it, and the embryo may now frequently be seen doubled upon itself. At this stage it is ready to escape from the sporosac, which gives way for its exit, and the embryo may then be seen swimming through the surrounding water by the aid of the minute vibratile cilia which clothe its entire surface. It is of an elongated conical figure, but very contractile. When fully extended, its surface is smooth; but when contracted, it is thrown into transverse rugæ, which give it a close resemblance to an annuloid animal. The rugæ never show themselves on the thick extremity, which always continues smooth, even in extreme contraction.

Tubularia coronata.

In the *Tubularia coronata* (Van Ben.) the gonophores are borne upon the body of the polype immediately within the posterior circle of tentacula. They consist of a long blastostyle carrying numerous sporosacs, and are destitute of investing capsule. The zoophyte is diœcious. In referring to my notes of this species made some years ago, but which I have not since had an opportunity of verifying, I find that the phenomena presented by the development of the ovum point to a type quite different from what prevails in that of the sertularian zoophytes, and in Clava, Coryne, &c. In the present species, the embryo is not the result of a transformation of the entire ovum, as in the instances just mentioned, but is produced from a definite portion of the vitellus, the remainder of the vitellus being absorbed by the developing embryo.

The embryo itself is developed on an entirely different plan from that of the sertularidans, &c. Instead of presenting the form of an elongated ciliated cone, destitute of all appendages, as in the latter, it assumes here somewhat that of two short thick cones placed base to base, surrounded at the place of contact by a circle of long filiform tentacula slightly thickened at the ends.

In this condition it leaves the sporosac, and by the aid of its long tentacula, moves about freely in the water.

As development proceeds, and apparently before it had left the gonophore, a mouth is found upon one apex of the double cone, and round this mouth a circle of short tentacula afterwards sprout out. In a further stage, we find that the opposite apex has become elongated into a hollow peduncle, by which the young Tubularia permanently fixes itself to some solid body, while the clavate condition of the extremities of the tentacula entirely disappear, and these organs acquire a uniform thickness throughout.

Little more is now needed to bring it into the form of the adult Tubularia.

Laomedea flexuosa. Hincks.

In this zoophyte the gonophores are of an oval form, generally truncated at the summit. They consist of a blastostyle, with sporosacs and investing capsule.

The blastostyle is in the form of a cylindrical column, expanded into a sort of head at its distal end, and having a distinct ectoderm and endoderm inclosing a central cavity, which freely communicates with that of the cœnosarc of the polype.

From the sides of the column numerous sporosacs bud forth, carrying with them an ectothecal investment from the ectoderm of the blastostyle, and being more mature the nearer they approach to the distal extremity of the blastostyle. They possess a large simple manubrium, and are destitute of gastro-vascular canals.

The whole is surrounded by the oval capsule. The formation of this capsule may be observed by watching the growth of the gonophore. In the young state of the gonophore one or more lacunæ may be seen in the ectoderm of the blastostyle. These become confluent, and soon extend round the whole blastostyle, thus separating the ectoderm into two distinct layers by a true process of chorization. The inner layer still remains adherent to the endoderm; but the outer layer recedes farther and farther from the central column, to which it remains directly attached only at the proximal and distal ends, thus forming the walls of an external capsule, whose axis is occupied by the blastostyle, and whose cavity is nothing more than a large lacuna. Into this lacuna the sporocysts bud forth from the sides of the blastostyle. While the gonophore is yet young numerous irregular fleshy bands may be seen stretching across the cavity from the blastostyle to the external wall. These bands are the remains of the original union between the two layers into which the ectoderm of the blastostyle has split. They are generally torn, and disappear as the capsule, increasing in size, becomes more and more widely separated from the blastostyle; but they are also occasionally more or less visible in the full-grown gonophore. In the meantime, the ectodermal layer, thus separated from the blastostyle, becomes invested by a distinct chitinous polypary; and after the capsule has acquired its full size, this ectodermal layer generally disappears along with the connecting bands just described, and the capsule is now solely represented by the chitinous secretion of its original wall.

Each sporosac (female) produces a single ovum, in which the germinal vesicle and spot are distinctly visible. The segmentation of the vitellus can be easily followed; and as the segments become smaller and more numerous, a nucleus may be distinguished in each. The ovum at the same time increases in size, and the manubrium of the sporosac becomes more or less displaced. After the disappearance of the mulberry-like condition, an external dermal layer becomes distinctly differentiated. It is composed of elongated cells, placed perpendicularly to the surface, and may be seen to enclose a minutely granular mass. The ovum, at the same time, becomes considerably elongated, and may be soon seen doubled on itself. It now acquires cilia on its surface, and is ready to escape as a free embryo from the sporosac, which accordingly becomes ruptured to allow of the exit of the embryo, which ultimately gains its final freedom through the summit of the capsule. The embryo now moves freely, by the aid of its ciliated surface, through the surrounding water. It is of a conical or pyriform figure, but very contracted and mutable. Its interior may be seen to be hollowed out into a cavity, but as yet no mouth can be demonstrated.

I have not yet succeeded in witnessing the change of the locomotive to the fixed state of the polype, but it is doubtless similar to what has been observed in the allied forms.

The male capsules and sporosacs resemble the female so closely as only to be distinguishable from them by an examination of the contents of the sacs. These contents consist of a mass of spermatogenous tissue, which replaces the single ovum of the female.

The spermatozoon consist of caudate corpuscles, about $\frac{1}{6000}$ of an inch in diameter.

Laomedea flexuosa is strictly diœcious, the male and female gonophores always occurring on separate colonies.

Antennularia antennina.

The gonophores in *Antennularia antennina* are borne upon the upper side of the short processes which, springing in verticils from the main stem, give support to the polypiferous ramuli.

Each process carries a single gonophore, which is of an oval form, and presents, as it approaches maturity, a subterminal aperture directed towards the main stem of the zoophyte.

The gonophore is constructed on the compound type, and presents a blastostyle, sporosacs, and investing capsule. The sporosacs are given off near the base of the blastostyle; there is usually but a single one; occasionally, however, two may be observed in one gonophore. The manubrium is well developed, but there are no gastrovascular canals.

In the female sporosac a single ovum makes its appearance. This at first occupies but a small portion of the cavity of the sporosac and permits the long manubrium to be easily seen, but as it grows it entirely fills the sporosac, and ultimately, by its pressure, causes the absorption, first of the walls of the sporosac, and at last of the manubrium, and blastostyle, until nothing remains but the external chitinous envelope of the capsule, with the ovum floating freely within it.

I have also frequently observed, floating along with the ovum in the gonophore, a small free sporosac with well-developed manubrium, but containing neither ova nor spermatozoa. It was probably a bud, formed, like the ordinary sporosacs, from the blastostyle, but never developing within it the generative elements.

The male gonophores resemble the female in all respects except in the contents of the sporosacs, which are here spermatozoa, instead of ova. In the young gonophores the sporosac is filled with semi-fluid contents, which are found to be composed of a mass of cells, frequently with secondary cells, "vesicles of evolution," in their interior. The secondary cells, whether free or contained in the mother-cells, are filled with a corpuscular fluid, in the midst of which may generally be demonstrated a larger corpuscle, which under the action of acetic acid is rendered especially apparent as a bright spherical nucleus.

The contents of the sporosac, which were at first sufficiently transparent to admit of the manubrium being clearly seen in the midst of them, become more and more opaque as the gonophore advances to maturity, and finally completely conceal the peduncle. If the contents be now liberated by rupture of the sporosac, they will be found to consist, partly of free active spermatozoa, and partly of cells (vesicles of evolution), with the spermatozoa still confined in them. The spermatozoa consist of a minute oval or rather pyramidal body with a delicate caudal filament. In each of the vesicles of evolution there may be distinctly seen a somewhat elongated nucleus, which is the body of the spermatozoa, as yet confined in its vesicle of evolution, and is plainly derived from the original spherical nucleus of the vesicle.

The ovum, from the earliest period at which I have observed it, appears as an opaque yellowish body. From an early stage it may be seen to consist of a mass of minute spherical cells filled with a yellow fluid, while the whole is enveloped in a delicate vitelline membrane. In the young ovum the germinal vesicle may be seen as a single large spherical cell included in the midst of the other contents, whose opacity, however, makes it necessary to subject the ovum to compression before we can bring into view the germinal vesicle, which may then be completely isolated as a separate cell on the field of the microscope. In the more advanced ovum the germinal vesicle has entirely disappeared; but I did not succeed in satisfactorily tracing any very distinct segmentation, owing, doubtless, to the unusual opacity of the ovum. The whole ovum becomes gradually converted into the embryo. As the time approaches when it is to leave the gonophore, we find it capable of changing its form by slow contractions, and it soon escapes by the aperture of the gonophore, and enters on the external world as a free embryo.

It is now of a more or less conical form, though continually changing its shape by slow contractions. By this time the ectoderm and endoderm are both differentiated, and a central cavity has already made its appearance; but there is as yet no trace of a mouth; thread-cells, the characteristic product of the ectoderm, are copiously developed in it, and its surface is clothed with very minute cilia, which, however, in all the examples I examined, were so enveloped in a mucous investment as to impede their action, and render them powerless as organs of locomotion. The embryo creeps about slowly upon the sides of the glass jar in which it is confined, avoiding the light side of the vessel.

After enjoying for a period its locomotive stage, the embryo fixes itself to the side of the jar by one extremity (the wider?) which then extends itself by means of radiating elongations into a little disc of a regular stellate form. From the centre of the free surface of the disc a cylindrical column now rises perpendicularly, and the whole becomes invested, at this stage, with a delicate transparent polypary. The column continues to grow longer, and now presents at intervals a shallow constriction. The cœnosarc which fills its axis is at first a simple tube with its endoderm and ectoderm; but it soon becomes resolved into the distinct tubules which characterize the cœnosarc in the stem of the adult.* The currents in these tubules are very evident, but are quite independent of one another—sometimes they may be all seen running down, sometimes running up: some down in one or two tubes, up in the others, sometimes the current will be very active in some, and at rest in the others.

From the basal disc small tubular filaments are prolonged to constitute the commencement of the matted root-like base of the mature colony.

From the parts of the stem where the constrictions show themselves, short thick processes are shot out alternatety at each side, so that the stem now presents a slightly zig-zag form. From the upper side of each process a pair of the peculiar little cup-like organs, characteristic of the adult zoophyte, are produced, and on its extremity the first joint of the polypiferus ramulus makes its appearance. This joint is soon followed by another, and the ramulus gradually elongates itself by the necessary multiplication of joints. We have now a condition of the zoophyte very remarkable from the fact of its polypiferus ramuli presenting a strictly alternate arrangement, no tendency to the verticillate disposition of these ramuli in the adult being yet apparent.

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* In the main stem of the adult, the disposition of the cœnosarc is very peculiar. Instead of forming a single tube, it consists of numerous separate tubules, each with its ectoderm and endoderm. The tubules lie close upon the polypary, and leave an unoccupied space in the axis of the stem. They are connected to one another by an extension of the ectoderm, which thus forms a continuous lining of the polypary. In some parts of the tubules of the cœnosarc run straight and parallel to the axis of the stem, in others, they are more or less curved, and frequently connected by transverse but irregular branches, so as to present a reticulated arrangement. The motion of the contents of the tubules can be distinctly witnessed in them. This complex structure of the cœnosarc disappears in the ramuli, the separate tubules here giving place to the ordinary simple tube.

Beyond this point I have not yet been able to follow the development of the young Antennularia.

Campanularia caliculata. Hincks.

I obtained this species on the 24th September 1857, from rock pools near low-water mark in Courtmasherry Harbour, with the gonophores. I obtained it afterwards in considerable quantities towards the end of the following October, from the same locality, adhering to *Delesseria sanguinea*, brought up on the long lines of the fishermen, but it was then almost entirely destitute of gonophores.

The gonophores are borne on the creeping stolon, to which they are attached by a short peduncle. They are of an irregular oval shape, with the summit truncated.

The blastostyle, in every case I examined, carried one large sporosac, which occupied about the upper two-thirds of the gonophore, and one smaller, and less developed, springing from the blastostyle near its base.

The sporosacs present some interesting peculiarities. The manubrium is obsolete, but four gastrovascular canals extend from the base of the sac to the summit, where they terminate in blind extremities. These canals send out short, lateral, alternate branches between every two of which, in the female sporosacs, an ovum is embraced. The germinal vesicle and spot are distinctly demonstrable, and the ovum is itself invested by a delicate membranous sac, which confines it in the sinus between the branches of the gastrovascular canals.

I had no opportunity of observing the development of the ovum.

Plumularia pinnata.

The gonophores, which are compound, are of an oval form, sometimes smooth, sometimes with a few irregular spiny longitudinal ridges. They are borne on the central stem or rachis, chiefly towards its attached end.

The blastostyle is but moderately developed, and carries usually only a single sporosac; but I have occasionally met with two or three. The manubrium, after advancing for a short distance into the sporosac, becomes much, but irregularly, lobed. Into these lobes the cavity of the manubrium is continued, and they may be fairly taken to represent the gastrovascular canals, which have no further equivalent in this species. In very young sporosacs the manubrium appears quite simple, but as the sporosac advances towards maturity the lobed condition becomes apparent.

The ova vary in number. I have occasionally found but a single one in each sporosac, though most usually from three to eight. They present the germinal vesicle and germinal spot, and may be observed to undergo segmentation.

The ciliated embryo is of the usual conical form. When about to change to the fixed state it attaches itself by one extremity, which becomes extended in the form of a four-lobed star, resembling a Maltese cross, from whose centre rises perpendicularly the primordial stem of the future zoophyte, at first in the form of a small cylindrical process, which elongates itself more and more, becoming at the same time invested with a delicate polypary.

We next find that on one side of the young stem a cell is formed in which the cœnosarc becomes developed into a polype.

Beyond this point I had no opportunity of observing the progress of development.

I have found the male gonophores on the same stem with the female, so that here the usual diœcious condition is departed from. The male gonophores are smaller and much less numerous than the female. The manubrium is less distinctly lobed, and is surrounded by a mass of spermatozoa instead of ova. The spermatozoa consist of a minute, somewhat pyramidal body, about $\frac{1}{5000}$ of an inch in diameter, with a caudal filament. They are developed in vesicles of evolution, from which they seem to be produced by a transformation of the nucleus.

I obtained the *Plumularia pinnata* in abundance with the reproductive capsules, during the months of September and October, in rock pools near low-water mark at Lisnaleen.

Plumularia cristata.

Plumularia cristata is very remarkable, by a singular arrangement destined for the protection of its gonophores.

These are borne on certain peculiarly metamorphosed ramuli, which we must be careful not to confound, as has hitherto been done, with the proper gonophores of other zoophytes, and for which, believing it therefore necessary to give them a special name, I propose the term *corbulæ*, suggested by their basket-like form. In these corbulæ the proper gonophores are contained. The peculiar metamorphosis of the ramulus, which results in the formation of a corbula, consists in its developing from its sides alternate leaflets, which have their edges at first entire, but which afterwards become deeply serrated. As the leaflets increase in size they direct themselves vertically from the upper surface of the ramulus, and those of one side arch over, so as to approach those of the opposite. They are at first free, but they afterwards become intimately united at their edges, while those of one side ultimately coalesce with those of the other by their summits, and thus form a completely closed receptacle. Each leaflet contains a cavity which is only a prolongation of that of the ramulus.

In this receptacle the gonophores are produced. They spring from the upper side of the metamorphosed ramulus at the point where the leaflet leaves it, and take the place of the polype cells on an ordinary ramulus. They begin to be produced at an early stage of the corbula, and may be easily examined in the young corbula while yet open.

The metamorphosed ramulus generally remains unchanged for a short distance from its origin, and here may be seen bearing one or two ordinary polype cells.

About twelve gonophores are generally contained in each corbula; they are of the simple type, of a regular oviform figure, and are invested with a delicate extension of the polypary. The sporosac has a well-developed manubrium, which is quite simple, and extends nearly from the base of the sac to the summit. I have not found more than a single ovum in the female sporosacs I examined. In the male sporosacs, the cavity is filled with the spermatogenous tissue.

General Conclusions.

A comparison of the different forms of gonophore presented by the several species just described, and by some others not included in the present paper, shows that they are referable to three distinct types.

1. The *simple gonophore*, such as we find in Hydractinia, Cordylophora, &c.

2. The *naked compound gonophore*, consisting of blastostyle and spo-

rosacs, but destitute of investing capsule. Examples of this form are found in Tubularia and Clava.

3. The *capsular compound gonophore*, consisting of blastostyle and sporosacs, and having the whole invested by a distinct capsule. This type occurs in Campanularia, Laomedea, &c.

The gonophores may present a further remarkable condition, in having a number of them grouped together, and included in a common receptacle formed by modified ramuli, as in *Plumularia cristata*.

Besides the above types, certain interesting modifications of form are presented by the sporosacs.

In these the manubrium may be

1. A simple diverticulum from the cœnosarc or the blastostyle, as in Hydractinia, Laomedea, &c.

2. It may be irregularly lobed, as in *Plumularia pinnata*.

3. It may send off from its base true gastrovascular canals, as in Cordylophora.

4. It may be completely suppressed, while well-developed gastrovascular canals spring from the base of the sporosac. This condition we find in *Campanularia caliculata*.

In the development of the embryo, we are probably justified in distinguishing two distinct types, though further observations will be needed before we can consider the generalization involved in this assertion as absolutely established.

1. The embryo may be developed directly from the whole vitellus, and will then always (?) present the form of a ciliated conical body.

2. The embryo may be developed directly from only a part of the vitellus, and will then always (?) present the form of a non-ciliated actiniform body.

2. *On the Focal Adaptation of the Eye in Man and some Animals*, By Dr James Black, F.G.S. Illustrated by enlarged diagrams.

3. *Note on the Black Lustrous Varnish of Ancient Pottery.* By John Davy, M.D., F.R.SS. L. & E., &c.

So far as my reading extends, the nature of the black varnish of the ancient Greek and Etruscan vases is still undetermined.

From the experiments I have made, operating on very small quantities, abraded from vases which were taken from tombs in the Ionian Islands, I have been led to the conclusion, that it is a vitreous matter, coloured by black oxide of iron, probably mixed with particles of metallic iron, to which its peculiar lustre may be owing.

It is, I find, of the hardness of glass, brittle and opaque. In powder or small fragments, it is powerfully attracted by the magnet. Before the blow-pipe it is fusible, its colour remaining unchanged, however powerfully it may be urged by the flame. It is insoluble in the nitric and muriatic acids, and also in the nitro-muriatic, and without change of colour; but, when fused with boracic acid, and then acted on by muriatic acid, its colouring matter is dissolved, siliceous matter remaining, and the solution is slightly precipitated by ammonia.

Considering this glazing as a compound of silica and of an alkali, or of an alkaline earth, coloured by iron, it may, I presume, be inferred, that it was applied to the earthenware in the form of a paste, and that the vessels were afterwards subjected to a temperature sufficiently elevated to melt the paste, and convert it into glass, but not high enough to fuse the substance of the pottery, which I find is fusible at a very high temperature. It may also, I think, be inferred, that the ferruginous colouring matter was mechanically mixed with the paste, before being applied;—

an inference I am led to, from the circumstance, that where the varnish is very thin, it is no longer opaque,—the red colour of the clay is seen through it; and, on minute inspection with a magnifying glass, evidently owing to a partial absence of the black colouring matter.

Probably the ancient vases, of superior quality, in which the red colour of the clay is so finely contrasted with the shining black of the varnish, were subjected to heat, in close vessels, as the Turkish pipe-bowls, which are of similar material, and of the same pure red colour, are baked at present. They are placed in a dome made of clay, from which the air is excluded, the fire being heaped up around.

The extraordinary durability of this varnish, as remarkable as its beauty, entitles it, I cannot but think, to consideration; and it is chiefly with the hope of calling attention to the subject, especially the attention of those engaged in our porcelain manufactories, now carried on with so much science and taste, that I venture to communicate this note to the Society.

Monday, 18*th. January* 1858.—The Right Rev. Bishop TERROT, V.P., in the Chair. The following Communications were read:—

1. *On the Mechanism of the Knee Joint.* By Professor GOODSIR.

After alluding to the comparatively superficial manner in which physiologists, with the exception of the brothers Weber, have hitherto investigated the structure and movements of the joints, the author gave an abstract of the general results which he had formerly obtained in an examination of the knee-joint, made with reference to Meyer's valuable observations. He had found that, as stated by Meyer, the thigh and leg rotate on one another in opposite directions, at the close of extension, and at the commencement of flexion; and that the co-ordinated movements in the patella, the ligaments, and muscles, correspond generally with the account given by that observer; but in addition he had ascertained what had previously escaped notice,—

1. That the articular surfaces of the femur, tibia, and patella, are not continuous but faceted surfaces.

2. That in consequence of this faceted configuration, and the peculiar manner in which the opposite articular surfaces move on one another, they are in no position of the joint congruent throughout, but gape more or less in different parts of their extent.

3. That in addition to their lubricating function, the so-called Haversian glands, or fatty folds of the synovial membrane, are arranged with reference to the resulting gaps or chinks between the opposite articular surfaces, each gap, as it opens out, being simultaneously occupied by the fatty synovial pad provided for it, and which is forced or dragged into the chink, and pulled or forced out again by special arrangements.

The author next proceeded to state, as introductory to the mechanism of the knee-joint, the results which he had latterly obtained in his examination of other diarthrodial articulations.

1. All diarthrodial surfaces are faceted, and consist of areas of distinct configuration and movement.

2. These facets and areas are marginal or terminal, and central or acting—the former giving steadiness to the action of the joint, and supplying surface on which it rests securely at the opposite extremities of its movements—the latter more especially regulating the movements themselves, and presenting the greatest extent of surface, which again consists of a moiety for each half of the movement, the one portion breaking contact while the other is acting, and *vice versa.*

3. Even the acting facets of opposite articular surfaces are only congruent at one particular stage of their movement.

4. The movements of opposite diarthrodial surfaces upon one another appear to be in every instance a combination of gliding and rolling—the amount of the former being directly, and that of the latter inversely, as the congruence of the opposite articular surfaces.

Referring to the important simultaneous discovery recently made by Langer and Henke, and verified by Meissner, of the screwed structure of certain joints, the author proceeded to state that he would, in a future communication on the ankle and tarsal joints, give the grounds on which he had come to the conclusion,

1. That in all the joints hitherto examined, the screw is developed on a conical surface, and not on a cylindrical one, as is held by Langer to be generally the case.

2. That not only is it impossible accurately to prolong the screwed surface by uniting longitudinally a number of casts made from it, but that neither the original surface nor its cast admits of being screwed along the mould with continued congruity of surface.

3. That this incongruity depends, in the first place, on the screwed surface being conical, and on the rapid increase in the obliquity of the thread; and, in the second, on its consisting of at least two areas, each being a portion of a conical screw.

After exhibiting prolonged screws made according to Langer's method, from the upper articular surface of the astragalus in the horse, panther, lion, and human subject, the author proceeded to state, that, induced to re-examine the knee-joint from this fresh point of view, he had ascertained, in the first place, that the path described by any point in the thigh, when the leg is fixed, and the knee put through its movements, does not lie in the presumed plane of flexion and extension as it would do if the profile curvatures of the femoral condyles were circular arcs or logarithmic spirals, according to the ordinary view, or that of the brothers Weber; neither does the point in the upper part of its course describe the arc of a circle in a plane oblique to that in which it must afterwards move, if Meyer's observations be absolutely correct, but, on the contrary, describes a helix, consisting of at least two parts, an upper and a lower. This observation led the author to the detection of two screw combinations in the knee-joint; and by a careful study of the anatomical relations of the elements of the articulation, he came to the following conclusions,

1. The knee-joint consists essentially of two conical screw combinations.

2. One of these screw combinations forms the anterior, the other the posterior part of the joint.

3. The axes of these screw combinations, instead of being at right angles to the so-called plane of flexion and extension, as in the ankle and elbow joints, are parallel, or nearly so, to the axis of the limb, the vertices of the fundamental cones being directed upwards.

4. The femoral condyles form the concave; the tibial condyles, intercondyloid spine, and crucial ligaments, the convex elements of each screw combination.

5. Each of these screw combinations is double-threaded; the breadth and obliquity of the threads rapidly increasing from vertex to base.

6. A comparatively limited extent of the convex element of each combination is retained, so that the larger extent of the concave element employed moves on the former by a combination of gliding in the direction of the screw, and of rolling.

7. The gliding in the direction of the screw is due partly to the screwed

configuration of the opposite cartilaginous surfaces; partly to the peculiar mode of attachment of the crucial ligaments.

8. In consequence of the peculiar attachments of the successive fasciculi of the crucial ligaments, these fasciculi, after having in succession cooperated in producing the gliding movement in the direction of the screw, bend over, and thus permit the rolling movement.

9. The path described by any point in the thigh or leg during flexion or extension of the knee joint is a helix, produced by the movements of the two screw combinations in succession: but modified by the rolling.

10. The anterior screw combination is left-handed in the right knee, and right-handed in the left.

11. The posterior screw combination is right-handed in the left knee, and left-handed in the right.

12. The two screw combinations in each knee are united, so that the anterior half of the anterior combination, and the posterior half of the posterior, are alone retained; while the external femoral and tibial condyles respectively consist of the united basal portions of one of the threads in each combination; and the inner condyloid surfaces respectively of portions of the other thread in each, but consequently towards the vertices of the fundamental cones.

13. When the knee-joint is fully extended, its anterior screw combination is screwed home, and its posterior is unscrewed; when it is completely flexed, the anterior combination is unscrewed, and the posterior screwed home.

2. *On the Exhibition of both Roots of a Quadratic Equation by one Series of Converging Fractions.* By Edward Sang, Esq.

Monday, 1st February 1858.—Sir David Brewster, V.P., in the Chair.

The following Communications were read:—

1. *On the Form and Origin of the Symbols on the Ancient Sculptured Stones of Scotland.* By Dr Wise.

2. *Notes on the Structure of Amphora, a genus of Diatomaceæ, and the diagnosis of its species.* By Dr Walker Arnott.

When Linnæus said that all objects of natural history must have a *specific name*, he did not mean a *trivial* name (which was not then invented), but what is called a short, distinctive character, otherwise it is not imperative on others to adopt the trivial name imposed, or recognize it in any way. The want of short characters (intended to place clearly before the mind the few essential points of difference between supposed new and already known forms or species) cannot be supplied by figures or diffuse descriptions of the entire object, as these leave quite in the dark the precise *marks of distinction* observed by the writer, if such actually existed. In composing either a defining character or a detailed description, it is also necessary to use the technical language of that science. The author, in referring to Dr Gregory's paper on the Diatomaceæ of the Clyde, published in the last part of the *Transactions*, regretted that this patient observer had neglected these rules, and thus enveloped his whole memoir in an almost impenetrable cloud; thus not only precluding himself from claiming any right of priority of names, in the event of the same form being afterwards correctly characterized by another under a different name, but depriving the paper itself of its claims to be considered a scientific one. The same unfortunate cloud rendered it difficult to understand what Dr Gregory's actual views of the structure of Amphora were; although, from expressions used by him, he appears to enunciate the theory, that what other writers call a simple frustule, ought to be considered as a double one.

The author, to make this more intelligible to those not generally interested in such pursuits, defined what the structure of a diatom was, as is explained by Smith in his Synopsis of British Diatomaceæ; and indicated the mode of proving, by Canada balsam, whether the frustule was single or double. When tested in this way, what was *commonly* called a simple frustule was found to be actually so, and of one cell, so that Dr Gregory's hypothesis was untenable. The structure of the genus Amphora appears to have been also slightly misunderstood by Kützing and Smith. The real form of the frustule is not a spheroid, as they must have considered it, but rather like that of a coffee-bean, rounded at the back and hollowed out in front, the line connecting the two terminal and central nodules of each valve being the median line; this line and the central nodule are thus not marginal, as hitherto described, but exactly as in other diatoms in which such are found. An Amphora would thus chiefly differ, by the half of the valve on the one side of the median line being concave, while the other was convex; whereas, in most genera of the group the two halves of the valve are precisely alike.

The form and structure of the frustule being established, the parts capable of affording good distinctive marks for species were next examined. All naturalists agree, that if these are taken from variable parts, they must be of less importance than if derived from those that are subject to little or no variation; and that no observation can be relied on, of a permanent kind, when taken from parts known to change their appearance rapidly. Thus the zone connecting the two valves of a diatom, which, from being a mere line, is understood to attain the whole breadth of the frustule in the course of twenty-four hours, has been deservedly rejected; and hence it is to be feared that few or none of Dr Gregory's species of "Complex Amphoræ," which owe their peculiar appearance to it, will stand the test of diagnostic characters. As the striæ, costæ, or furrows, are the same on both sides of the median line, and as the valve is folded, those at the back of the frustule must be seen through the medium of the surface nearer the eye, and crossing those belonging to it, so that observations on these relate entirely to the *accidental* position the frustule happens to be in. This compels one to depend chiefly for essential characters—1*st*, on the small portion that is seen between the median line and the apparent outline of the frustule; and, 2*d*, on the form of the frustule itself, *previous* to the siliceous connecting zone commencing the process of self-division.

The author also stated his conviction that no certain conclusions could be drawn as to what was a new form or species from deposits or dredgings, on account of the impossibility of procuring the species in an isolated state, and consequently of studying them independently; the same species putting on very different aspects, and different species assuming the same aspect at particular stages of self-division.

Microscopical differences are by themselves of little importance. To see is one thing, to understand and combine what we see, another; the eye must be subservient to the mind. Every supposed new species requires to be separated from its allies, and then subjected to a series of careful observations and critical comparisons. To indicate *many apparently* new species is the work of an hour, to *establish* only *one* on a sure foundation is sometimes the labour of months or years. In microscopical natural history as much scrutiny is required to prove a new form to be distinct from its allies as in chemistry to discover a new alkaloid, or in astronomy to demonstrate the identity of two comets. A naturalist cannot be too cautious. It is better to allow diatoms to remain in the depths of the sea, or in their native pools, than, *from imperfect materials*, to elevate them to the rank of distinct species, and encumber our catalogue with a load of new names so ill defined, if defined at all, that others are unable to recognize them; the same object can be more easily attained by attaching them, in the meantime, to some already recorded species, with the specific character of which they sufficiently accord. In all such cases the question to be solved for the advantage of naturalists is not, whether the object noticed be a new species, but whether it has been proved such, and clearly characterized.

Monday, 15th February 1858.—Mr James T. Gibson-Craig, Treasurer, in the Chair. The following Communications were read:—

1. *Description of the Sulphur Mine near Conil; preceded by a Notice of the Geological features of the southern portion of Andalucia.* By Dr Traill.

2. *Remarks on a Slab of Sandstone containing numerous Cavities, apparently produced by Marine Animals.* By Charles Maclaren, Esq.

3. *Notice respecting some Artificial Sections illustrating the Geology of Chamouni.* In a Letter from John Ruskin, Esq., to Professor Forbes.

Royal Physical Society.

Wednesday, 25th November 1857.—W. H. Lowe, M.D., President, in the Chair.

It was announced that the Rev. Professor John Fleming, D.D., late President of the Society, had died on the 18th November.

Dr Lowe, the President who retires, delivered an Address, bearing on the present state and prospects of the Society in those branches of science more particularly cultivated by its members.

The following communications were made:—

1. *Contributions to the Natural History of the Hudson's Bay Company's Territories.* Part I.—Reindeer. By Andrew Murray, Esq.

   (This paper appears in the present number of this Journal).

2. (1.) *On Reproduction by Ova from the Medusoid of* Campanularia Johnstoni. (2.) On *Ephelota coronata, a new Protozoan animalcule.* By T. Strethill Wright, M.D.

   (This paper appears in the present number of this Journal).

Wednesday, 23d December.—Professor Balfour in the Chair. The following communications were read:—

1. *On the Skull of a Wombat from the Bone Caves of Australia, with a few general remarks on the Marsupiata.* By James M'Bain, M.D., R.N.

After some preliminary remarks upon the first discovery of marsupial animals—the opossums in America, and afterwards the kangaroos in Australia, during the first voyage of Captain Cook, it was stated that upwards of seventy species have already been found on the Australian continent. That recent species also inhabit Tasmania. New Zealand, and several islands of the Indian seas. Fossil remains have been discovered in the Stonesfield slates, near Oxford, belonging to the Lias formation. One species of Didelphis (*D. Cuvierii*) in the Montmartre gypsum, near Paris, and at least five genera from the bone caves of Wellington Valley in Australia, have been found in a fossil condition. Dr M'Bain then gave a detailed description of the skull in his possession. He was induced to exhibit the present specimen to the Royal Physical Society, although the absence of crania from the public museums had precluded him from being able to determine whether this skull belonged to a recent or an extinct species. The length of the skull, on its upper coronal surface, measured from the anterior tips of the nasal bones to the perpendicular crest of the occiput, was 6 inches and 7-10ths of an inch. The greatest breadth, from the upper edge of the zygomatic arches was 4 inches and 8-10ths of an inch. With regard to the composition of the cranium, the original elements constituting the four occipital segments of the skull present a flat, vertical surface, and are united by continuous ossification. This anchylosed condition also applies to other sutural connections. In front, the malar bone becomes flattened from above downwards, until the upper and lower margins nearly join, thus forming a broad, concave space for the eye to rest upon. This peculiar form of zygoma bears an evident relation to the horizontal flattening of the upper and under surface of the skull, and greatly diminishes the resemblance which in many respects exist between the rodent order of placental mammals and the wombat. A narrow ridge, two inches in length, directed from within outwards, forwards, and upwards, and slightly concave transversely, forms the articular surface for the lower jaw. This remarkable structure of the glenoid cavity permits of free movement in every direction, and in this peculiarity of mechanism it differs from the kangaroo and koalo amongst its congeners, and from the rodent order of placental quadrupeds. Instead of the usual vertical compressed form of this portion of the lower jaw, a strongly depressed horizontal form is observed, with deep hollow spaces for the insertion of the masseter muscles externally, and the internal pterygoids within, two muscles which, along with the external pterygoids inserted into the sigmoid notch before the condyles, and the temporal muscles surrounding the coronoid process, are those chiefly concerned with the movement of the jaws in the act of mastication. A large foramen is seen at the outside of the posterior mental foramina, probably for the transmission of nerves from the inferior maxillary branch of the fifth pair, to the largely developed masseters; which usually pass, along with the blood-vessels, over the sigmoid notch. The characters afforded by the teeth were next briefly described.

2. *Note on the Discovery of Hematite Iron Ore, on the Garpel, Ayrshire.*

By ALEXANDER ROSE, Esq., Lecturer on Geology and Mineralogy.

The deposit of red hematite on the Garpel is of great extent. It was examined many years ago by Rose, who reported on the quantity as large, and the quality as excellent; the latter he stated, according to the analysis of red hematite, by Daubuisson, to consist of—

Protoxide of iron, . . .	94·0
Silica, ...	2·0
Lime, ...	1·0
Water, ...	3·0
	——100

This protoxide would yield, of— .	Per cent.
Metallic iron, ...	66·17
Oxygen, ...	28·83
Silica, lime, and water, . .	6·0
	——100

This ore he found to be in four states,—1st, In mammillated or uniform shapes; *2d*, massive; *3d*, granular; *4th*, pulverulent—the two latter conditions being the effect of disintegration. The reniform structure is concentric, scaly; internally, compact or fibrous. The streak is deep red, which is characteristic. In all its conditions and characters it corresponds with the long-used hematite of Ulverston in Lancashire, and of Whitehaven. The yield of the Garpel ore is, in round numbers, double that of blackband ironstone. It has, however, remained disregarded and useless until lately.

3. *On the Skeleton, Muscles, and Viscera of the Malapterurus Beninensis.* By John Cleland, M.D.

Dr Cleland read a paper on the detailed anatomy of the skeleton, muscles, and viscera of the *Malapterurus Beninensis*, the new electric fish from Old Calabar; in which he pointed out marked differences between this species and that from the Nile, in the shape and number of the bones. The anatomy of the electric apparatus was left to a more favourable opportunity.

4. *Note on an Artesian Spring, which has lately appeared on the banks of the Almond, near Wester Whitburn, Linlithgowshire.* By Andrew Taylor, Esq.

The spring was found as a bore was being prosecuted in a field on the banks of the Almond, opposite the Red Mill, and midway betwixt Easter Whitburn and Blackburn. When nearly seventeen fathoms depth had been reached, water copiously gushed out of the bore-hole, and was conducted in a tube seven feet above the surface.

Wednesday, 27th January, 1858.—Andrew Murray, Esq., President, in the Chair. The following communications were read:—

1. *Introductory Report on the Natural History of the Pearl Oyster (Meleagrina margaritifera,* Lam.) *of Ceylon.* By E. F. Kelaart, M.D., Trincomalie. Communicated by R. Kaye Greville, LL.D.

He stated the following results of his observations:—The pearl oyster is more tenacious of life than any bivalve mollusc I am acquainted with. It can live even in brackish water, and in places so shallow, that it must be exposed for three or four hours daily to the sun, and other atmospheric influences. That it has locomotive powers, beyond any idea which can be formed from former observations. That the power of moving from place to place is inherent, and absolutely necessary, in early life, for the due performance of the animal functions. This is obvious from the fact, that if a cluster of young oysters stayed permanently in one place, adhering to each other, the growth of the animal, and particularly of its shell, would be prevented. That the pearl oyster will move about in search of food, if the locality in which it is originally placed is not rich in its natural supplies. That it will move from its original situation if the water becomes impure, either from the decomposition of vegetable or animal matter, or muddy; and, probably too, if there is a large influx of fresh water. That if the water is agitated to an inordinate degree, the oyster will leave its old mooring place and seek another. That a thunder-storm will kill some in an aquarium. That the animal can unfix itself from its byssus; and that crabs, shrimps, and other creatures force it to form a new byssus, by nibbling through the old one. That it can re-form its byssus at pleasure, if in good health and condition. That it can live for a long time without forming a byssus, and that it will re-form a byssus when it has recovered strength. That the power of re-forming its byssus is not confined to the young animal; but that a very large living oyster can re-form it in an aquarium, as well as in the depth of the sea, but not so actively as the young and middle aged. Pearl oysters are gregarious in their habits. In placing several young oysters in different parts of an aquarium, they will sooner or later be found attached to each other. The older ones have also this desire, but their heavy shells impede their motion, and they are contented to remain apart from their fellows. That, taking the foregoing facts into account, there appears to be no reason why pearl oysters should not be translated from their native beds, and made to colonize other parts of the sea. That the young, as well as the old, are in spawn from March to September, and that probably there is no stated period for spawning. The whole occupation of the oyster, when fixed to a spot, appears to be, keeping its valves open, and admitting food to its mouth. For several hours the valves remain open; they then close for a few minutes, or for an hour or two, then open again. At night the valves remain generally open till towards daylight, when they close, and remain so till the sun shines brightly over the horizon. It is during the early part of the night, or soon after sunset, that they exercise, *when required*, their locomotive powers.

2. *Exhibition of Lignite from the Ballarat Gold Field, and of some specimens of recent Woods from Australia.* By William Oliphant, Esq.

3. *Entomological Notes for* 1857. *Lepidoptera.*—By R. F. Logan, Esq. *Coleoptera.*—By Andrew Wilson, Esq. (Specimens were exhibited.)

4. (1.) *On the Genus Peltogaster (Rathke); an animal form parasitic on the abdomen of Crabs.* (2.) *On the occurrence of the* Galathea Andrewsii. By John Anderson, Esq.

The distribution of these parasites in Europe appears to be very wide, especially that of *Peltogaster Carcini*. Specimens of this species have been obtained from the coasts of the Mediterranean, Crimea, Norway, and from the "Black Banks" in the North Sea. Professor Bell was the first to notice it on our coast; and I have also to record it as a British parasite. The *Peltogaster Paguri* does not appear to be so extensively distributed as the former species. Rathke, who first described it, obtained his specimens from the Norwegian coast; Kroyer appears to have met with it in the Kattegat. This species has not been previously recorded as a British animal. The invariable position of this parasite is on the left side of the abdomen of the crab it infests, immediately below the false feet. The colour is orange yellow, tinged with copper green. All organs of sense are absent, and it presents the appearance of a well-filled sack tied at the mouth. It forms a moderately curved oval, terminated in a short snout, which opens into the cavity of the body. The disc by which the Peltogaster attaches itself is of a horny consistence, and is situated in the middle of the body. It is star-like in form, and by means of it these parasites are so deeply rooted into the body of the crab in which they live, that they are unable ever to leave their position. Dr T. S. Wright states that, through the opening which exists in the centre of the disc there issues a tube, which, in making its appearance in the body of the crab, breaks up into an innumerable quantity of copper-coloured tubules, which ramify through the whole body of the unfortunate Bernhardus. I have detected these tubes passing to the base of the antennæ, ramifying through the claws, thorax, and abdomen, and giving to the soft and transparent parts of the crab a well-marked greenish hue. The function of these tubes is evidently to supply the body of the parasite with nourishment.

2. *On the Occurrence of the* Galathea Andrewsii.

This little Galathea, which was figured and described by Professor Kinahan in the Proceedings of the Nat. Hist. Soc of Dublin for 1856–7, I dredged in Shetland in considerable numbers last August. It is therefore to be considered as a Scottish species.

Wednesday, 24th February.—WILLIAM RHIND, Esq., President, in the Chair. The communications read were as follows:—

1. (1.) *Notice of a new Lizard,* Matricida lugens (Murray), *said to be venomous, from Old Calabar.* By ANDREW MURRAY, Esq.

(2.) *Observations on the Metamorphoses of Orthopterous and Hemipterous Insects (supplemental to previous communication on the Leaf Insect,* Phyllium Scythe). By ANDREW MURRAY, Esq.

2. *On the Skull of a Seal from the Gulf of California; with some preliminary observations on the Amphibious Carnivora.* By JAMES M'BAIN, M.D., R.N.

In the skull of the common seal (*Calocephalus vitulinus*), which represents the Inauriculatæ, there is no post-orbital process; the mastoid can scarcely be called a process, and seems to form a part of the large rounded tympanic bulla. It is separated from the bulla by a distinct groove, extending from the *stylomastoid foramen* obliquely backwards and inwards. In the section to which the *Calocephalus vitulinus* belongs, there is no trace of an ali-sphenoid canal. In the Auriculatæ there is a distinct post-orbital process and an ali-sphenoid canal; the mastoid process is largely developed, and stands apart from the tympanic bulla, which is small and projecting. The carotid canal has the same direction as in the Canidæ, while in the common seal it enters farther forward, and does not again appear externally. The orbito-sphenoids are greatly compressed in front, so that the optic foramina seem to have coalesced into one. These osteological characters have afforded a basis for the division of the Phocidæ into three sub-families. 1. *Arctocephalina;* a post-orbital process; a distinct ali-sphenoid canal; mastoid process strong and salient, its surface continuous with the tympanic bulla. This sub-family contains two genera—*Otaria* and *Arctocephalus.* 2. *Trichecina;* no post-orbital process; an ali-sphenoid canal; one genus and one species only known, *Trichecus Rosmarus* (Walrus). 3. *Phocina;* no post-orbital process; no ali-sphenoid canal; mastoid process swollen, and seeming to form part of the tympanic bulla. By examining this Californian skull, it will be seen that the osteological characters correspond to those ascribed to the sub-family Arctocephalina. There is a well-developed post-orbital process and a distinct ali-sphenoid canal. The mastoid is a strong, irregular-shaped process, deeply impressed by the muscles attached to this portion of the cranium. The carotid canal commences directly in front of the *foramen lacerum jugulare,* and again appears at the anterior part of the tympanic bulla. The *foramen condyloideum* is situated at the posterior and inner margin of the *foramen jugulare;* in the common seal it is placed farther back, and directly behind the foramen. The orbito-sphenoids are so strongly compressed in this specimen, that the optic foramina have literally coalesced into one. The posterior palatine foramina are small, and chiefly confined to the palate bones. A striking peculiarity of this skull, by which it can scarcely fail to be distinguished from any other species, is an elevated sagitto-coronal crest, extending in a gradually arched form from the upper edge of a parieto-occipital crest until it diverges by two slight ridges and loses itself in the post-orbital processes. This crest is rather more than an inch high in the centre, and indicates the presence of powerful temporal muscles in this species. The nasal process of the superior maxilla reaches as far back as the posterior extremities of the nasal bones. In the common seal, the nasal process does not pass so far backwards.

3. *On the Discovery of Beekite and Oolitic Quartz at Durness, Sutherland.* By Charles William Peach, Esq., Wick. (Specimens of Beekite and Oolitic Quartz were exhibited.)

Botanical Society of Edinburgh.

Thursday, 12th November.—Professor Balfour, V.P., in the Chair. The following communications were read:—

1. *Short Account of a Botanical Trip in the Island of Arran, with Pupils, in 1857.* By Professor Balfour.

2. *Notice of Abnormality in Flowers of Lilium.* By J. CHRISTIAN, Esq. Communicated by WILLIAM BRAND, Esq.

3. *Short Notice of a Peculiar Form of Fungus.* By JAMES YOUNG, M.D.

Specimens of the fungus were exhibited. It is an imperfect state of Coprinus, and was found by Dr Young, while assisting Mr Edwards in the operation of excision of the knee-joint. The patient (an Irishman) was, after the operation, laid on a new and clean bed, with a hair mattress, which had been previously covered with gutta percha sheeting, and the limb supported by a M'Intyre splint. The patient lay in considerable comfort for some days. The bed, however, became very soon damp, and it was found necessary to have him changed. On the fourteenth day after the operation, he was removed from the bed to a sofa till the mattress was changed, and a new one substituted, when our attention was directed to an extraordinary appearance on the under part of the bed, where the fungus was produced, in large quantity, growing equally from the spar as from the mattress. The bed was thoroughly cleaned, but in spite of this, at the expiry of nine or ten days, the same appearance was again presented, the fungus being nearly in equal quantity as before.

4. *Remarks on the Fungus.* By the Rev. M. J. BERKELEY. Communicated by Professor BALFOUR.

The fungus is an imperfect state of some Coprinus. A similar case is reported in some Italian Transactions, and I recollect one which occurred at St George's Hospital in 1825, and the occurrence was much commented upon at the inquest. The Hospital authorities on the inquest chose to deny the fact; but I recollect seeing the case in the Hospital before death, and was requested to say what the species was. I never, however, saw the specimen, as by some accident it had been destroyed. The treatise to which I allude is entitled :—"Sopra alcuni funghi ritrovati nell' apparecchio di una frattura." Modena. 4to, 1805. Targioni —Tozzetti.

5. *Remarks on the Microscopical Structure of Cotton Fibre, with reference to* Mr GILBERT J. FRENCH's *proposed Improvements in Spinning.* By GEO. LAWSON, Ph. D.

6. *Notice of the Discovery of a New Station in Britain for* Polygonatum verticillatum. By the Rev. W. HERDMAN. Communicated by Professor BALFOUR.

The station is Drimmie Burn Den, near Glen Ericht Cottage, parish of Rattray. Mr Herdman states that it was found at Strone of Cally by Dr Barty, some years ago. It has also been long known at Craighall ; and the Drimmie station is nearly intermediate in position between these two places, which are about four miles apart. At the same place there is abundance of *Paris quadrifolia.*

7. *An Account of some of the Habitats of the* Polygonatum verticillatum. By Dr JAMES RATTRAY.

8. *Notice of the occurrence of* Asplenium viride, *on a wall near Arno's Grove, Southgate, Middlesex.* By V. EDWARD WALKER, Esq. Communicated by Professor BALFOUR. Specimens were shown.

Thursday, 10*th December* 1857.—Dr Seller, President, in the Chair.

The following communications were read:—

1. *Notice of Egyptian Plants.* By Dr John Kirk.

Dr Kirk gave a short account of a tour in Egypt and Syria during the spring of 1857, and exhibited specimens of the more interesting plants, as the *Ficus Sycamorus*, *Mimosa Lebbek*, and *Acacia nilotica;* this is one of the gum-yielding trees. *Acacia Seyal* was said by the natives in the upper country to yield no gum, but to be used for charcoal. *Cassia ovata* was grown near Assouan; this forms a small part of the Alexandrian Senna. The camel loads which they lay on the sand at Assouan for transport to Boulak were all found to contain nothing but lanceolate leaflets of good quality, and free of Tephrosia and Cynanchum. The plants observed in cultivation were the sugar-cane, cotton, rice, wheat, maize, shoura, indigo, lablab, phaseolus, cicer, vetches, lupins, castor-oil, and tobacco. Each village had a group of date palms, and often *Zizyphus Spina Christi.* The dhom palm was not observed further north than 28°. Dr Kirk observed that the weather on the Nile had been very variable and cold last season, yet invalids in general were very much improved during their residence in the upper country. In Syria the spring was late, so that few flowers had appeared except in the rich valleys near Tiberias, which were covered with a profusion of beautiful plants. In the north, that is between Beyrout and Damascus, the mulberry is the great source of wealth; the olive, vine, apricot, and walnut, are also grown; in the south, the cereals, vine, and olive. The oranges are very fine at Jaffa, whence they are taken to Constantinople.

2. *Notice of Plants found in the neighbourhood of Comrie, Perthshire.* By Mr D. P. Maclagan.

3. *Contributions to Microscopical Analysis.* No. 1. Tobacco. By Dr George Lawson.

In this paper Dr Lawson called attention to the imperfect descriptions that existed of the histological characters of tobacco, and the consequent liability to error in microscopical analysis on the part of those who depended upon books for their knowledge. It has been customary to characterize the tobacco as distinguished by its hairs being "glandular," or having an "enlargement" or "roundish swelling" at the tips; but this very imperfectly indicates the peculiar structure of these hairs, which, although extremely variable in size and general form, present certain characters in their lower cells, and in the structure of the glands at their tips, which are very constant and of great practical value. These characters were shown by a series of microscopical drawings from various species of Nicotiana, as well as from manufactured tobacco. The characteristic hair of the tobacco leaf varies from 1-20th to 100th of an inch in length, and is generally thick and gouty at the base, and tapering towards the extremity where the glandular structure is placed; that structure is of an oval or rounded form, and consists of a few closely-packed but well-defined cells, which are very much shorter than the other cells of the hair. The elongated cells of the body of the hair (of which the lower one is most characteristic on account of its very large size), contain fine colourless granular matter, and generally nuclei; but the secreting cells are well furnished with colouring matter of a reddish-brown, but sometimes of a green colour. A one-inch object glass, recommended by Hassall for the examination of tobacco, is usually insufficient to show the *structure* of the gland, and the mere presence of "glandular hairs" proves nothing, these being common in plants. It is also necessary to keep in view that many small hairs occur on tobacco leaves, which are normally without glands. The glandular hairs are most abundant at the tips of the shoots, and especially on the calyx and flower stalks of the tobacco. Dr Lawson, in calling attention to the remarkable prevalence of glandular hairs on the surface of plants in many families, observed that we have here a striking illustration of the view which he endeavoured to explain to the Society last summer, viz., that the secreting structures of plants are invariably formed by *epidermal cells*, even where these structures are deeply imbedded in the plant's tissue. To the fact that epidermal hairs are so frequently organs of secretion, Gasparrini has recently added the additional one, that they are also organs of absorption.

6. *Notice of Galls found by Mr Beveridge on the leaves of the Beech.* By Mr James Hardy. Communicated by Professor Balfour.

Thursday, 14*th January* 1858.—Dr Seller, President, in the Chair.

The following communications were read :—

1. *On the Occurrence of a New Muscari on Mount Ida.* By Dr J. Kirk.

The author remarked—In April 1856, a party was formed among the officers stationed at the British Hospital of Renkioi, on the Dardanelles, for the ascent of Mount Ida, about forty miles distant, in a south-easterly direction. At first our route was over a rough country, through the villages of Renkeny and Doumenek, as far as the old Roman aqueduct, crossing a ravine in the metamorphic rocks. This had been constructed to supply Novum Ilium with water. Many of the old clay pipes are now used as chimneys to the native hovels. The stream which flows through this ravine is named the Kemar-son, and joins the river Mendere, but in the heat of summer it is lost in the sand about a quarter of a mile from its junction. Thus far the ground had been covered with brushwood of *Pistacia Terebinthus*, *Storax*, *Pinus Halepensis*, and dwarf oaks of several species. A few clumps of handsome Valonia oaks, *Q. Ægilops* and *Q. Cerris* were seen towards the plain of Troy. In the valleys the Oriental plane, the poplar, and prickly Paliurus bush grew luxuriantly, festooned with *Clematis cirrhosa* and *Vitalba*, *Periploca græca*, *Cynanchum*, and wild vine. The ground was covered with several species of anemone, iris, and crocus. After having crossed the Kemav, we soon entered a pine forest covering the high grounds as far as the plain of Beyramitsh. Next morning, we followed the Mendere through a rich well-watered valley. By the road sides the hop and lint grew wild. *Anemone appenina* and *Scilla bifolia* were picked; they had been transported from a higher region by the waters. Between this and the foot of Ida the country was rough and barren, intersected by ravines, through which the Scammander found its way to the plains. At the village of Avjylar we had some difficulty in obtaining lodging. Early the following morning we began to ascend on foot. Proceeding in an oblique direction for some time we came to one of the sources of the Scammander, where it gushes by many powerful springs from the schist rocks. In this neighbourhood we found Saxifrages, Geraniums, *Dentaria bulbifera*, *Ruscus Hypoglossum*, and *Pæonia decora* among the fine timber of *Pinus Pinaster* which covered this region. There, too, Muscari was picked in considerable abundance, which seems to be a new species, and which we have named, from its remarkably broad leaves, *M. latifolium*. It now appeared that our guides had deceived us, and taken us off the proper road, and from this point it seemed almost impossible to ascend. But, being determined to reach the top, we set off, leaving them to follow if they chose. Near the summit the forest opened out, and left nothing but bare rock; we picked *Crocus garganicus*, *Corydalis tuberosa* and *digitata*, *Viola gracilis*, *Scilla bifolia*, *Ornithogalum nanum* and *fimbriatum*. The scanty soil had been turned up by the wild pigs in search of bulbous roots. The ascent had occupied from 7 in the morning till 3 P.M. On our return we followed a much easier path, and here we found *Saxifraga sancta* growing in wet boggy spots. This species had been previously discovered by Griesbach on Mount Athos. The sun had set by the time we reached the village of Avjylar, and, having enjoyed a night's rest, we set off on our return to the Hospital, where we arrived on the fifth day from our departure. Dr Kirk briefly indicated, in the following terms, the characters of the new Muscari, which will be more fully described before he leaves for the Zambesi river:—*Muscari latifolium*; scape erect, about 12 inches in height, rising from a globose bulb, and bearing near its base a large sheathing, broadly lanceolate, rather obtuse, solitary leaf; flowers numerous, forming a raceme about two inches in length, the lower ones shortly pedicellate, the upper ones barren, sessile; perianth tubular (blue), in the fertile flowers inflated below. *Muscari latifolium*, Armitage, Kirk, and Playne, in Herb.

2. *Note on* Cryphæa (Daltonia) Lamyana, *Montagne*. By Dr George Lawson.

Dr Lawson stated that in 1836 M. Montagne had described and figured, in apparently a very careful manner, a new moss found near Vienna, under the name of *Daltonia Lamyana* (*Ann. des Sc. Nat.* 2 *serie. Botanique*, tom. 6, pp. 327–329, tab. 18, fig. 2). Subsequent writers had referred it to *D. heteromalla*. Specimens shown to the meeting, which had been collected in the river Taw by the Rev. C. A. Johns were considered by Mr Wilson and others to be identical with M. Montagne's moss; but they differed so widely from his elaborate description, that Dr Lawson thought the whole subject was still deserving of inquiry. The points which remain to be determined are these:—1. Is *D. Lamyana*, Montagne, a good species? 2. Is the English plant identical with it?

3. *On the correspondence between the Serial Internodes of Plants and Serial Crystalline Forms.* By Mr Wm. Mitchell. Communicated by Professor Balfour.

4. *On* Macadamia, *a new genus of* Proteaceæ. By Dr G. Lawson.

5. *Recent Botanical Intelligence.* By Professor Balfour.

List of Herbaceous plants and shrubs, in flower in the open air, at the Royal Botanic Garden, Edinburgh, 14*th January* 1858. By Mr James M'Nab.

Veronica Buxbaumii, Veronica Andersonii, Symphytum caucasicum, Ruta graveolens, Bellis perennis, Chrysanthemum sinense, Sisyrinchium grandiflorum, Hepatica triloba varieties, Phlox verna, Primula Auricula, Primula vulgaris, Primula veris, Gentiana acaulis, Viola odorata; Scrophularia annua, Leontodon Taraxacum, Iberis sempervirens, Tussilago fragrans, Tussilago alba, Vinca major, Vinca minor, Aponogeton distachyon, Anchusa sempervirens, Galanthus nivalis, Helleborus niger, Helleborus graveolens, Helleborus laxus, Helleborus olympicus, Helleborus atrorubens, Potentilla alba, Potentilla fragarioides, Alchemilla conjuncta, Alchemilla montana, Cheiranthus Cheiri, Matthiola incana, Erysimum Perowskianum, Cydonia japonica, Rhododendron atrovirens, Rhododendron Nobleanum, Jasminum nudiflorum, Garrya elliptica, Erica herbacea, Erica stricta, Viburnum Tinus, Arbutus Andrachne, Arbutus serratifolia, Arbutus Unedo, Corylus Avellana, Cornus mascula, Camellia japonica, Daphne Mezereum, Arabis premorsa, Arabis iberica, Alyssum gemenense.

In reference to Mr M'Nab's list, Dr Balfour called attention to the remarks of Dr Lindley, who states that, in accounting for the vegetation of 1857, attention ought to be directed in a special manner to the heat of the soil. Little has been done as yet in the way of obtaining accurate accounts of the temperature of the earth at the depths of one and two feet during the period of vegetation. In April 1857, the ground heat was nearly 3° higher than usual. In May it was 1° 23′ warmer than usual. The earth heat continued to advance very much in June, moderately in July. It was also augmented in September, October, and November. In the latter month, at the depth of two feet, it was warmer than usual by nearly 7°. During eight important months, the earth at one foot below the surface had absorbed 29° 26′ more than usual, and even at two feet 12° 20′. Add to this the remarkable fact that in November the heat at one foot below the surface was within 2° equal to that in May, and we may explain the ripening of many exotic fruits this season. The geo-thermometer explains satisfactorily the phenomena of last season.

Thursday, 11th February 1858.—Dr SELLER, President, in the Chair.

The following Communications were read :—

1. *Remarks on the Sub-order Orthotricheæ.* By BENJAMIN CARRINGTON, M.D., Yendon, by Leeds. Communicated by Dr GEORGE LAWSON.

2. *Notes of a Botanical Trip with Pupils to Coldstream and Norham in July* 1857. By Professor BALFOUR.

3. *Remarks on the Distribution of Plants in the Northern States, Canada, and the Hudson's Bay Company's Territories, &c.* By Dr GEORGE LAWSON. Part I.

4. *Notice of the Produce of the Olive Crop in the Island of Corfu during the past Season, in a Letter from* Mr MACKENZIE, *Corfu, to* Dr GEORGE LAWSON, *dated January* 11, 1858.

Mr Mackenzie observes,—"Whatever may have been the disturbing cause, it is evident the unusual state of the weather and temperature in this island since April last has arisen from some uncommon electrical deviation, or at least that this was the chief agent. On the 17th of October, we were visited by a severe hail storm, followed by a beautiful water-spout, having the appearance of a huge inverted funnel. A gentleman crossing the Channel at the time in an open boat compared the hailstones to pieces of brick. Yet it is remarkable that this extraordinary season seems to be peculiarly favourable to the olive crop, which is exuberant and exceeds that of any year since 1833. The olive is at all times a precarious crop, requiring different degrees of temperature at different stages. In the green state, heat is necessary; in September, when the colour becomes red, moisture is indispensable; and, in the last stage, to preserve the olive from a destructive insect, produced by foggy and sultry weather, a cool and clear atmosphere is absolutely necessary. This mischievous insect eats its way round the kernel, and the drupes gradually decay and drop. The sirocco wind is the chief agent in this process, a few days' continuance of its sultry breath being sufficient to destroy an abundant crop.

5. *Remarks on a species of Loranthus, and Measurements of Tree Ferns in Australia.* By Mr Thomas Cannan. Communicated by Professor Balfour.

6. *Notice of Plants collected in the Isle of Skye.* By Dr John Alexander Smith and Dr Gilchrist. Communicated by Dr George Lawson.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Gutta Percha of Surinam.—Professor Bleckrod of the Delft Academy, has recently given a notice of the gutta percha of Surinam. Although gutta percha has been known in Europe for a dozen years, and has now come into general use, yet much still remains to be done regarding it, both as respects its uses and its sources. The Professor states that Dutch Guiana can supply gutta percha. This is of importance, when we consider the value of the article, and the probable exhaustion of it in the countries from which it is now supplied. The Dutch Government took measures to transplant the *Isonandra Gutta* and cultivate it in Guiana; but they have lately discovered in that country a species of Sapota, to which Blume gives the name of *Sapota Mulleri*, which yields a juice in every way equal to that of the Isonandra. It is probable that other trees of the same natural order may be found to yield a similar product. *Achras Sapota*, the fruit of which is known in the West Indies as Neesbery, also yields a milky juice like gutta percha. *Sapota Mulleri* of Blume is probably the tree called "Bullet-tree" by the English, and its wood is known as "horse-flesh." It is a tall tree, yielding in summer a large quantity of milky juice. It appears that under the name common Boerowe, or Bullet-tree, there have been confounded; 1, the *Lucuma mammosa* of Gærtner (Marmalade tree)—the Mimusops of Schomburgk; 2, The white Boerowe: which is the *Dipholis salicifolia* of Alph. D.C., and is known in Jamaica as Galimata; 3, The bastard Boerowe or Lowranero, which is the *Bumelia nigra* of Swarts; and 4, the Neesbery Bullet-tree, or *Achras Sideroxylon* of botanists, which yields one of the best of the Jamaica woods. *Sapota Mulleri* grows abundantly on slightly elevated situations. In collecting the milk the trunk is surrounded with a ring of clay, with elevated edges, and then an incision is made in the bark as far as the liber. The milky juice flows out immediately, and is collected in the clay reservoir. The juice resembles in some respects the milk of the cow. It forms a pellicle on its surface, which is renewed after removal. By the evaporation of the juice, we obtain 13 to 14 parts in 100 of pure gutta percha. This Surinam gutta percha is said to be sold at Amsterdam at the same price as the best gutta percha of commerce.

Urtical Alliance, as divided into Orders. By H. A. Weddell.

Filaments of the stamens straight in præfloration,	Flowers hermaphrodite or polygamous,		*Ulmaceæ.*
	Flowers unisexual,	Herbs with a watery juice, leaves, at least at the base of the stem, opposite,	*Cannabinaceæ.*
		Trees or shrubs commonly lactescent, with alternate leaves,	*Artocarpaceæ.*
Filaments of the stamens inflexed in præfloration,		Ovule anatropal, pendulous,	*Moraceæ.*
		Ovule orthotropal, erect,	*Urticaceæ.*

Auguste Trecul *on the presence of Latex in the Spiral, Reticulated, Barred, and Dotted Vessels of Plants.* (Ann. des Sc. Nat., 4me. ser. tom. vii., p. 289.)

Observers have hitherto looked upon latex as contained only in laticiferous vessels. Trécul, on the other hand, maintains that this fluid is found also in spiral, reticulated, barred, and dotted vessels. These vessels, according to him, elaborate the fluid, and distribute it through all parts of the plant. The latex assumes different colours; sometimes it is white or milky, at other times yellow or orange, at other times colourless. Trécul has selected plants with yellow or orange-coloured latex as subjects of experiment, such as *Chelidonium majus, C. quercifolium Agemone ochroleuca* and *A. grandiflora.* This coloured latex is not found in all the vessels at once, nor even in all parts of the same vessels. There are great varieties in this respect. The coloured juice seems to be modified by the physiological action of the vascular tissues. The latex at certain times, as late in autumn, disappears from the spiral vessels, and becomes collected in the laticiferous vessels. Similar results were observed in plants with milky latex, as *Ficus Cariça, Morus alba*

Euphorbia Characias, *E. prunifolia*, &c. He thinks that the latex is secreted by the spiral vessels and their modifications, and is afterwards received as an excretion by the laticiferous vessels. At the same time it appears that starchy matter is formed in the latter vessels, and is from them transferred into the other vessels in immediate contact with them.

In *Carica Papaya*, he shows that small ramifications of the laticiferous vessels are prolonged to the surface of the reticulated vessels and terminate there. He is disposed to look upon the laticiferous vessels as analogous to the venous system of animals. He is confirmed in this view, by considering the place which the laticiferous vessels occupy in the midst of tissues where the greatest vital activity prevails,—the chief constituents of this juice being formed of substances little fitted for immediate assimilation, since they are carbo-hydrogen (caoutchouc), or slightly oxygenated products (resins, alkaloids, morphia, narcotine, codeine, &c.), which are produced from a sap already used for the purposes of nutrition. He thinks that thus carbo-hydrogen, and resins, and alkaloids, may be oxidated or more fully elaborated in the vessels, in order to return and take part in the production of starch, sugar, albuminous matter, and cells.

These observations, he says, explain certain phenomena which have puzzled physiologists, viz., why plants absorb carbonic acid during the day and reject it during the night. There is a constant passage into the vessels, and there takes place during day and night, among other chemical reactions, a true oxidation in their interior. Plants take the oxygen of the air for the purposes of this combustion, and they reduce it to the state of carbon during day as well as night; but during the night the carbonic acid is exhaled, whilst during the day it is decomposed by the influence of light before being sent out, its carbon being fixed, and its oxygen only eliminated. The respiration of plants thus consists of two phenomena:—

1. An absorption of carbonic acid during the day with the emission of oxygen.

2. An oxidation in the vessels at the expense of the oxygen of the air, with the formation of carbonic acid during the day as well as during the night, but with the exhalation of this acid during the night only, because during the day it is decomposed in passing through the leaves.*

Thus respiration and circulation in animals and plants have a greater analogy than is usually supposed. The laticiferous vessels represent the venous system, and the vessels properly so called the arterial system. Hence M. Trécul calls the former *venous vessels*, while the spiral, reticulated, barred, and dotted vessels are denominated *arterial vessels*.

In the case of plants which have laticiferous vessels and no true vessels, it would appear that the cells take the place of the latter; and in cellular plants, having neither laticiferous nor other vessels, the cells perform the functions of both.

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* These views are not new. They were first brought forward by Professor Burnett, and have been supported by Dr Carpenter. They are fully detailed in the writings of both these authors, as well as in works on vegetable physiology, as Balfour's *Class-Book of Botany*, page 467.

Vegetation Around the Volcanic Craters of the Island of Java.

By M. H. Zollinger.

Decandolle, in his *Geographie Botanique*, has omitted to notice among vegetable stations that around volcanic craters. In Java there are more than sixty of these craters, all isolated and surrounded by vast virgin forests. When the craters are active and send forth lava (which is not the case with the Java volcanoes), or cinders, or sand and fragments of rock, or when they exhale continually vapours and gases, then there is no vegetation, except some Oscillarias, which are found in hot-water springs. It is only when the direct volcanic action is diminished by the effect of time, or the distance of the crater, that a special vegetation appears. The craters of the Indian Archipelago are characterised by the absence of all parasitic or epiphytic plants, as well as of climbing and twining plants. Woody plants only appear at a considerable distance from the craters. We can easily distinguish three different regions —1. An interior zone, nearest to the centre of volcanic action. 2. A middle zone surrounding the first. 3. An exterior zone.

I. *Interior Zone.*—This exhibits mostly small species scattered here and there, belonging to the lower orders of plants, and to those having no corolla. Among them are, *Oscillaria labyrinthiformis*, Ag?, in warm springs; *Cladonia macilenta*, Hoff., and *C. bacillaris* or *obtusa* of Schær.; some fungi belonging to the genus *Polyporus*; a *Marchantia*; two or three species of mosses; some ferns, such as *Selliguea Feei*, Bory, *Polypodium triquetrum*, Bl., *Asplenium macropyllum*, Bl., *Asplenium mucronifolium*, Bl., and *Gleichenia vulcanica*, Bl. Among Cyperaceæ, *Phacellanthus multiflorus*, Steud. *Polygonum corymbosum*, Bl., is the only Dicotyledon.

II. *Middle Region.*—Many social ferns occur here, some Dicotyledons, for the most part small shrubby plants. Among the ferns are:—*Polypodium Horsfieldii*, R. Br. (3000—8000 feet), *Pteris aurita*, Bl., *Blechnum pyrophyllum*, Bl., *Gleichenia ferruginea*, Bl., *Mertensia longissima*, Kze., *Lycopodium spectabile*, Bl., *L. trichiatum*, Bory. We also meet still with *Phacellanthus multiflorus*, a Carex, *Polygonum corymbosum*, and *Imperata arundinacea*. A species of *Antennaria* and *Anaphalis* among Compositæ, and certain Ericaceæ appear; also, *Leontopodium*, *Elsholtzia elata*, *Wahlenbergia lavendulæfolia*, DC., *Ophelia javanica*, Hassk., *O. cærulescens*, Zoll., *Melastoma setigerum*, Bl, (the cells of which are said by M. Zollinger to contain crystals of pure sulphur), *Medinilla javensis*, Bl., *Rubus lineatus*, Reinw., besides other genera and species.

III. *Exterior Region.*—This region gradually loses itself in the ordinary forest vegetation. Some rare mosses, ferns, and orchids appear at the outer portion of the region. Among other plants may be noticed *Synæcia* (Ficus) *diversifolia*, Miq., *Rhododendron javanicum*, Reinw, *Agapetes elliptica*, Don, &c. Among the common arborescent plants may be mentioned, *Agapetes varingiæfolia*, Don, *A. myrtoides*, fem. *Myrsine avenis*, Bl. The beautiful *Albizzia montana*, Bth. (a social plant), *Casuarina montana*, Lesch, and *C. junghuhniana*, Miq., are on the outer part of the region. We find also here an arborescent *Boehmeria* and a dwarf *Epilobium*. Some twining plants form transition species, such as *Nepenthes gymnamphora*, Bl., and some varieties of *Polygonum corymbosum*. The order Ericaceæ is the predominant one; we find, beside the species already mentioned, *Rhododendron album*, Bl. (?), *Agapetes floribunda*, Don, and other species of the genus, *Gaylussacia lanceolata*, Bl., *Parnettia repens*, Zoll., *Gaultheria punctata*, Bl. (an odoriferous plant of great beauty), *G. leucocarpa*, Bl., and others; species of *Clethra* (?). The genus Rubus is well represented. *Dodonæa viscosa*, Andr. (?), is common towards the eastern part. The orchid that approaches nearest the craters is *Thelymitra javanica*, Bl.

These are the more common and more characteristic plants of the three crateric regions of Java, according to M. Zollinger.—(*Bibliothèque Universelle.*)

The Lotus or Sacred Bean of India.—Dr Buist gives some notes on the Lotus or Sacred Bean of India, in the Transactions of the Bombay Geographical Society. He says, "The lotus itself is one of the most elegant of eastern flowers, and seems from time immemorial to have been, in native estimation, the type of the beautiful. It is held sacred throughout the East, and the deities of the various sects in that quarter of the world are almost invariably represented as either decorated with its flowers, seated or standing on a lotus throne or pedestal, or holding a sceptre formed from its flowers, sometimes expanded and at others closed. It is fabled that the flowers obtained their red colour by being dyed with the blood of Siva, when Kamadeva wounded him with the love-shaft arrow. Lakshmi is called the lotus-born from having ascended from the ocean on its flowers. The lotus is often referred to by the Hindu poets. The lotus floating on the water is the emblem of the world. It is also the type of the mountain Meru, the residence of the gods, and the emblem of female beauty.

The lotus flower is repeated, *ad infinitum*, in the earliest Eastern sculptures as that on which Bhuddah sat, and from which Bramah sprung. In the Cave Temples of Salsette, dating back several centuries before the Christian era, it is represented everywhere at once as an emblem and an ornament.

Dr Buist thinks that Dr Lindley is mistaken in saying that the wicks used on sacred occasions by the Hindoos are made of the spiral vessels of the leaves of the lotus. They are formed, he says, of the dried flower-stack or leaf-stalk; he does not believe that all the spirals of all the lotuses in India, from the Himalayas to the line, would make a lump of wick a yard long the thickness of the finger. Individually the spirals are finer than gossamer; the leaf is fourteen by sixteen inches in diameter; the stalks about six by eight feet long, and seldom rise higher than two or two and a half feet above the surface of the water. The leaf is buoyant enough to support a crow, and is frequently made use of by that bird as a fishing station, from which flies, snails, or water lizards are preyed upon. The flower has something of the smell of the Tonquin bean, or the blossom of the bean. The upper surface of the leaf is a deep green. It repels the water when pressed under it. This is referred to in some of the native writings, a translation of one of which is given by Dr Buist:—

> "He is not enslaved by any lust whatever;
> By the stain of passion he is not soiled,
> As in the water, yet unwet by the water,
> Is the Lotus leaf."

When the leaf is held obliquely, the light is reflected as if from a mirror. The same thing occurs with drops of water thrown upon it, and this peculiarity can only be overcome by rubbing the leaf so as to destroy the fine texture, by which the result is brought about. This seems to consist of minute capitate papillæ, by which a fine film of air is kept entangled, the water in reality never coming in contact with the actual surface of the leaf at all—a fact established and illustrated by its reflecting light from its own under surface. The same phenomenon of repulsion of water is seen in the leaves of the *Pistia Stratiotes*, a floating plant abounding in shallow tanks in India. When pressed under water the leaves look like frosted silver. It is the same organisation that enables rose, clover, and young cabbage leaves, young shoots of grain and grass, and the numberless other plants that exhibit dew in its beautiful pearly form, to repel water from their surface. It is the same that produces the like results usually ascribed to oil and grease on the feathers of birds, especially of water-fowl, and most of all of divers—which when they plunge under the surface seem to carry with them a perfect flash of light. A piece of glass, a varnished or greased surface, or polished stone, throws the water off as perfectly as the various matters enumerated, but in none of these latter cases is there any appearance of reflection.

Dr Buist, on examining the lotus leaf in a little pool of water, noticed thin films of air arising leisurely and adhering to the leaf. The water flowing over them, by the reflecting light from its under surface, shows the area over which the air was emanating. The air gradually collected into bubbles and then rose to the surface. The quantity of air which rises is very great—especially from the spiral vessels when wounded in any way. A single stem of $\frac{1}{3}$ of an inch in diameter, containing tubes of a sectional area of not more than $\frac{1}{4}$ of this, or say, $\frac{2}{10}$ of an inch square, even where the leaf is cut off, has been ascertained by experiment to discharge 33 cubic inches of air hourly. The velocity with which the column advances must be at the rate of 20 feet an hour.

Hairs of Urticaceæ.—The stinging hairs (*stimuli*) of Urticaceæ consist of a single cell, more or less elongated, swollen at its base, where it is sheathed by a layer of epidermal cells, and terminated sometimes by a sharp point, but more commonly by a small rounded pyriform or acuminated knob. This hair becomes broken in the skin, and allows the acrid fluid it contains to flow out. This gives rise to accidents of a more or less severe nature. The severity of the sting depends not on the quantity of fluid which enters the puncture, but rather in its activity. The sheathing or bulbous portion of the hairs varies much in length. Sometimes it exceeds the free portion, as in *Urtica ferox*, one of the species, which gives a most dangerous sting. In some species of Urera and in one or two other genera, the sheathing portion increases much with age, becomes woody, and forms a true prickle or aculeus, analogous to those of the Rose, and of some species of Hibiscus.

Glandular hairs, properly so called, are rare among the Urticaceæ. Species of *Fleury* present examples of these hairs, as well as the ribs of the lower surfaces of *Parietaria communis*, on which we also notice, as in *Forskohlea* and some other genera, hooked or uncinate hairs. The species of *Forskohlea* or of *Droguetia* exhibit in different parts of their inflorescence, a mass of woolly hairs analogous to those which cover the cotton plant. None of the Urticaceæ have webbed hairs.

BOTANICAL BIBLIOGRAPHY.

An Elementary Course of Botany, Structural, Physiological, and Systematic; with a brief outline of the Geographical and Geological Distribution of Plants. By ARTHUR HENFREY, F.R.S., F.L.S., Professor of Botany in King's College.

This is an excellent botanical text-book, and is well fitted to give the student a clear and comprehensive view of the science of botany. It is divided into four parts—I. Morphology or Comparative Anatomy, including, 1. General Morphology; 2. Morphology of the Phanerogamia; 3. Morphology of the Cryptogamia. II. Systematic Botany, including, 1. Principles of Classification; 2. Systems of Classification; 3. Systematic Description of the Natural Orders. III. Physiology, including, 1. Physiological Anatomy of Plants. 2. General considerations on the Physiology of Plants; 3. Physiology of Vegetation; 4. Reproduction of Plants; 5. Miscellaneous Phenomena. IV. Geographical and Geological Botany.

The author is well known for his works on physiological botany, and for the able papers which he has written on the subject of vegetable reproduction. He has had extensive experience as a teacher, and has thus been able to form a judgment as to the best mode of instructing students during a three months' course of botany. The present work is founded on this experience. The author first directs the attention of the student to morphology, and then proceeds to systematic botany, leaving the microscopical structure of plants to be considered along with physiology. This he considers to be a better arrangement than that usually adopted in elementary books. The order pursued in text-books, however, does not appear to be of much importance. Every teacher who does not merely lecture, but who gives regular demonstrations in a garden and in the fields, and who, moreover, examines his pupils, finds that he must not be guided entirely by a book. He will vary his mode of instruction according to the capabilities and progress of his pupils, and will direct his efforts so as to make them understand the structure of plants, in the first instance, before proceeding to systematic botany. The object of the teacher is to familiarize the student with the organs of plants, so that he may be able at once to examine any flower presented to him, and to give an intelligent and philosophical view of its parts. In this way only can we expect parties to be trained for botanical investigation, and for examining the floras of other climes. The sciences of botany and natural history have been too exclusively regarded as departments of medicine. They ought to enter into the curriculum required for Masters of Arts as well as for Doctors of Medicine. Nay, they ought to constitute a part of the education of every gentleman. As regards medical students, these sciences should be part of the elementary training inculcated, and an examination on them should precede the entrance on more professional study. Medical students, in place of commencing with these sciences, too often defer them till the concluding years of their course, and thus completely invert the proper order of study. No wonder that in such circumstances they should be looked upon as irksome, seeing that they interfere, in some degree, with the time which should be devoted to medicine and surgery. We hope the time is not far distant when a preliminary examination of medical students will include not merely classics, physics, and modern languages, but also the natural history sciences to a certain extent.

Professor Henfrey's work is a valuable guide to the student, and will serve as an excellent groundwork for teaching. It contains many original views, and takes a clear and comprehensive view of the science. The author states that much care and labour have been expended on the preparation for, and execution of, the present volume, and he trusts that his experience as a teacher may prove to have enabled him to produce a good working text-book for the student, from which may be obtained a groundwork of knowledge in all branches of the science, without the attention being diverted from the more striking features of the subject and details comparatively unimportant. The author appears to have succeeded in his object, and to have produced a work worthy of his reputation.

Manual of the Botany of the Northern United States, including Virginia, Kentucky, and all East of the Mississippi; arranged according to the Natural System. By Asa Gray, Fisher Professor of Natural History in Harvard University. The Mosses and Liverworts by W. S. Sullivant. 2d edition, with fourteen plates illustrating the genera of the Cryptogamia.

This is a compendious flora of the northern portion of the United States, intended for the use of students and practical botanists. The author in speaking of the geographical region embraced in the work, states, that the southern boundary coincides nearly with the natural division between the cooler-temperate and the warm-temperate vegetation of the United States. The western limit, while it includes a considerable prairie vegetation, excludes nearly all the plants peculiar to the great western woodless plains. The northern boundary, being that of the United States, varies through about 5 degrees of latitude, and nearly embraces Canada proper on the east and on the west.

The natural orders are disposed in a series which corresponds nearly with De Candolle's arrangement, and there is given an artificial key to the natural orders, which enables the student readily to refer American plants to their proper family.

The work is one of great merit, and is indispensable for the student of the American flora.

First Lessons in Botany and Vegetable Physiology. By Asa Gray, Fisher Professor of Natural History in Harvard University, United States.

This is one of the best and cheapest elementary works on botany. It is intelligible to the general student, while at the same time it is scientific. It is the production of an author who is one of the ablest botanists of the day, who is a clear and an accurate writer, and a successful teacher. The book is intended for the use of beginners and for classes in the common and higher schools. It comprises an account of the structure, organs, growth, and reproduction of plants, and of their important uses in the scheme of creation. It is illustrated by upwards of 360 wood engravings from original drawings by Isaac Sprague, and it contains a copious glossary and dictionary of botanical terms. We recommend the book as one well calculated for schools.

Flore de Lorraine.—By M. Godron, Professor of Natural History of the Faculty of Sciences of Nancy. This elementary work gives a good view of the flora of Lorraine, accompanied with an account of the geology of the district, and of the soils in which the principal plants grow.

Hooker's Journal of Botany and Kew Garden Miscellany.—We regret that the December number terminated this journal, which has extended to nine volumes. It has been a most important vehicle for the conveyance of botanical information, and more especially for the description and enumeration of plants sent by various collectors. Its discontinuance we look upon as a great loss to the botanical literature of Britain.

ZOOLOGY.

Zoological Museum of Professor Van Lidth de Jeude.—The valuable museum of the Professor of Zoology and Comparative Anatomy in the University of Utrecht is announced for public sale on the 6th of April, and the following days. The catalogue of the first division, consisting of the mammalia, birds, and fishes, extending to 155 pages, is now published. Professor Van Lidth has no son or successor who would take an interest in this collection, formed during forty-two years labour in the University, and having now reached the age of seventy years, he thinks it prudent to dispose of it. The collection is a very large and valuable one; and besides the specimens in the catalogue now published, contains reptiles, and a series of specimens illustrating comparative anatomy, which will also be sold in August next. Mr H. E. Strickland, while on a visit to the continent in 1845, examined this collection, and thus writes regarding it—"Professor Van Lidth is an elderly man, and by a process of gradual accumulation has collected the largest general museum of zoology which I ever saw in the possession of a private individual. It includes a very valuable collection of Osteology, as well as of mammalia, birds, reptiles, fish, shells, insects, and zoophytes, all in the finest state of preservation, and well arranged in a large building constructed on purpose. The Professor assured me that his collection had cost him 200,000 florins." *

Cervus Euryceros, or Great Irish Elk.—A fine skeleton of this gigantic animal exists in the Museum of Edinburgh College, and its bones have been found in most parts of Europe, in peat bogs or other post-tertiary formations. Some naturalists have supposed that it lived within the human period, and was destroyed by the progress of civilization; but Professor Pictet, in the first volume of his *Palæontologie*, published in 1853, considers this opinion as not accordant with the geological position of its remains, and founded chiefly on observations made in Ireland. It appears, however, that evidence of the *Cervus Euryceros being contemporary with man* has been discovered in Switzerland. In partially draining a small lake near Moosedorf, in the canton of Berne, relics of human industry, fragments of pottery, arrow-heads, stone chisels, stakes cut to a point, &c., have been found in a lower part of a bed of peat, along with the bones of many domestic and wild animals,—the dog, cat, ox, sheep, bear, hog, fox, beaver, and, among others, an atlas and jaw, adjudged by the learned palæontologist himself to belong to the *extinct Irish Elk*. A paper on the subject was read by Professor de Morlot to the Imperial Geological Institute of Vienna in June last, and a brief notice of the paper has been published in the last number of the journal of the Geological Society of London.—*Charles Maclaren in Scotsman.*

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* Memoirs of Hugh Edwin Strickland, M.A.L. By Sir W. Jardine, Bart. Part I. p. 231.

Earliest Specimen of Animal Life.—It had been supposed that the earliest specimen of a distinctly organized animal to be found in the rocks, was a trilobite, till, about three weeks ago, a zoophyte of a very simple structure, named Oldhamia, was discovered at Bray Head in Ireland, in rocks previously deemed destitute of fossils. We are reminded, however, by Colonel Portlock's address to the Geological Society, that specimens of the *Palæopyge Ramsayi* were found by Mr Salter in the Cambrian rocks of the Longmynd, North Wales, which are quite as ancient as those of Bray Head. The trilobite is thus restored to its rank as the first created living being having a distinct and intelligible organization. Minute holes, *supposed* to be the burrows of sea-worms, were found along with the Palæopyge, but without anything to indicate their form or structure.—*Charles Maclaren in Scotsman.*

Supposed Antiquity of the Human Race.—In a paper read before the Royal Society, on 11th February, Mr Horner, giving an account of researches undertaken near Cairo, with the view of throwing light upon the geological history of the alluvial land of Egypt, stated that a fragment of pottery, now in his possession, an inch square and a quarter of an inch in thickness, the two surfaces being of a brick-red colour, had been obtained from the lowest part of a boring, 39 feet from the surface of the ground. The entire soil pierced consisted of true Nile sediment; and allowing the estimated rate of increase of deposited sediment of 3½ inches in a century to be correct, this fragment having been found at a depth of 39 feet, is the record of the existence of man of 13,375 years before A.D. 1858; 11,517 years before the Christian era; and 7625 years from the beginning assigned by Lepsius to the reign of Menes, the founder of Memphis—man, moreover, in a state of civilization, so far at least as to be able to fashion clay into vessels, and to know how to harden it by the action of strong heat.—*Athenæum.*

On the Electrical Nature of the Power possessed by the Actiniæ of our Shores.

By Robert M'Donnell, M.D.

After referring to the well-known phenomena manifested by electrical fishes, and to alleged instances of numbing effects, but of doubtful electrical nature, produced on the naked hand by the contact of certain marine Invertebrata, the author describes his own observations and experiments with the Actinia as follows:—

Suppose that into a vessel containing some actiniæ well expanded, and apparently on the look-out for food, some of the tadpoles of the common frog be introduced, these little creatures do not, like many fresh-water fishes of about the same dimensions, immediately die; on the contrary, the salt water seems to stimulate their activity,—they become very lively, and swim about with vivacity. One of them may not unfrequently be observed to make its way among the tentacles of an actinia, and get off again quite uninjured; it may even for a time nestle among the tentacles with as much impunity as if it were only in contact with a piece of seaweed; but should the tadpole have the misfortune to fall in with a more voracious actinia, the reception it meets with is very different. Sometimes, when by an incautious lash of its tail it touches even a single tentacle, it may at once be laid hold of, and in the violent efforts which it forthwith makes to break loose, often merely brings itself within the reach of other tentacles, by which it is seized and overpowered. Occasionally, however, after having been thus seized, the tadpole, by its superior activity, succeeds in effecting its escape, and when it does so, it seems for a time singularly excited; it twists and wriggles through the water, so as to leave no doubt that some very remarkable influence has been exerted upon it.

These observations are no doubt familiar to all who have studied the habits of these animals; for although the tadpole seems more susceptible of the peculiar stimulus which the actinia can communicate, than most of those creatures which are ordinarily cast in its way, yet the same occurrences take place with the small crustaceans, &c., which are abundant in sea-water. Indeed, no very close attention is necessary to perceive, that while on some occasions these little animals may creep to and fro, over the surface and among the tentacles of the actinia, at other times they are seized and killed with the greatest promptitude.

It remained to be determined what is the exact nature of the power which the actinia has been thus found to have under its control. If it seized its victim by a simple mechanical effort, why should the tadpole be so agitated for some time after having escaped from its grasp? No peculiarly viscid secretion could be detected on the tentacles, nor could any decided reaction be discerned on their surface differing from the feebly alkaline condition of the sea-water in which they were placed; moreover, the power of the actinia seemed often to be exerted with too much promptness to be compatible with the notion of the formation of a poisonous or stinging fluid over its surface.

On the hypothesis that it is an electrical power with which the actiniæ are endowed, it is obvious that the existence of animal electricity in them ought to be experimentally demonstrable by its physiological effects, inasmuch as these phenomena are the most striking which animal electricity is capable of producing in common with other electricities derived from different sources.

The following experiments, in which the frog's limb was used as a galvanometer (the limb of this animal being, as is well known, an instrument of extreme delicacy for this purpose), seem satisfactorily to establish the fact that the common actiniæ of our shores are gifted with electrical power.

1*st*, Having prepared the lower limb of a lively frog after the mode described by Matteucci, by stripping off the skin, dissecting out the sciatic nerve from among the muscles of the thigh, and then cutting off the thigh a little above the knee, so as to leave the nerve uninjured and as long as possible, the limb was laid on a small piece of glass, so that the nerve hung down over its edge. The pendent nerve was lowered into the water, and gently brought in contact with the tentacles of an expanded actinia. From the first, or the second, or even several, possibly no effect may result, but arriving at last at one more vigorous than his neighbours, smart muscular contractions follow as he grasps the nerve in his tentacles, and the toes are thrown into active movement.

2d, The next experiment, although of precisely the same nature as that first detailed, renders the effect produced on the muscles of the frog's limb more striking. A large and lively frog is killed, the skin is stripped off, and the viscera being removed, the body is cut off about the middle; a knife being slipped behind the lumbar plexus of nerves, the pelvic bones and contiguous soft parts are cut away, so that the lumbar vertebræ remain connected with the lower extremities merely by the nervous cords passing to each limb. Thus prepared, the limbs are laid on a thin piece of board, so that the vertebræ hang over its edge dangling by the undivided nerves. The piece of board is placed floating on the surface of the water in which are the actiniæ, and is slowly pushed over within reach of an active one. Immediately that the actinia seizes the morsel thus offered to it, contractions are observed to commence in the thigh, extend to the calf, and soon the toes are in movement.

3d, In order to set aside the supposition that these muscular contractions might be the result of chemical or mechanical irritation applied to the extremities of the nerves, it became necessary to devise a modification of the foregoing experiments; for although irritants, such as turpentine, croton oil, ammonia, friction with a nettle leaf, &c., were applied to the nerves without producing any effect like that obtained from the actiniæ, it seemed still possible that the contractions might be due to some other agent than electricity.

The following experiment seems to remove all doubt. A piece of copper wire, a few inches long, was coated with sealing-wax, except about half an inch at each end; the ends were rubbed clean with sandpaper, one of them was thrust into the lower part of the spinal canal of a frog prepared as in the last experiment, while the other, which was to be offered to an actinia, was passed into a portion of the frog's intestine put on like a glove; for the actinia does not seize vigorously metallic substances. The limbs of the frog, with the nerves and vertebræ attached, are laid on a piece of board, while the copper wire, which is curved, arches over the edge of it, so that the end covered with frog's intestine can be readily brought within the reach of the actinia. Having waited for a few minutes until the muscular contractions excited by thrusting the wire into the spinal canal have ceased (and they are in general very transient), the board is placed floating on the water, and the frog's intestine offered to an actinia; muscular contractions ensue, perhaps not so promptly, certainly not so vigorously as in the former experiments, but nevertheless easily to be recognised and unmistakeable. They commence in the thighs, and, as in the former case, extend to the calves, and then the toes move actively. This last experiment has been modified in a variety of ways, but the same result has been constantly obtained. Perhaps the best modification of it is to use a piece of copper wire, having one end coiled so as to form a disk, which is covered with chamois-leather, while the other is sharp-pointed to enter the spinal canal of the frog. The whole, except the surface of the disk, which is to be given to the actinia, and the point for the spinal canal, is covered with sealing-wax, and the frog's limbs extended upon a thin piece of board. With this arrangement precisely the same effects were produced as already described.

It is a remarkable fact, and deserves special notice, that in all these experiments the muscular contractions, when once strongly excited, whether by direct contact or through the medium of wire, do *not* at once subside. When the limbs are withdrawn from the influence of the actinia in the first experiments, or removed from the wire in the last, strong muscular contractions continue to take place for from three to five minutes.

All the varieties of actinia which have hitherto been made the subject of experiment, have given similar evidence of electrical power, but by no means in an equal degree. The large varieties are found, in proportion to their size, much feebler than those of less dimensions, and any attempt to succeed in the experiment with the copper wire has failed with them.

A somewhat similar observation has been made by Dr John Davy regarding the torpedo, for he tells us (*Philosophical Transactions*, 1834, p. 548) that he has seen strong vivacious fish which made great muscular exertions in the water, almost or entirely destitute of electrical action.

It is obvious that in creatures of such moderate dimensions as actiniæ, of so peculiar a form and of such feeble power, much difficulty is to be expected in demonstrating the other experimental effects which animal electricity is capable of producing in common with other electricities, viz., magnetic deflection,—magnetizing of needles,—spark,—heating power,—and chemical action; and it must be admitted that all experiments hitherto undertaken on this subject have been attended with negative results. I hope, and indeed expect, when further opportunities are afforded of examining these creatures in health and vigour in their native pools, to obtain more satisfactory results on these points, when I shall look forward to the pleasure of making a further communication on the subject.—(*Proceedings of the Royal Society of London.*)

GEOLOGY.

On some peculiarities in the Microscopical Structure of Crystals, applicable to the determination of the aqueous or igneous Origin of Minerals and Rocks. By H. C. Sorby, Esq., F.R.S., F.G.S.

In this paper the author showed, that, when artificial crystals are examined with the microscope, it is seen that they have often caught up and enclosed within their solid substance portions of the material surrounding them at the time when they were being formed. Thus, if they are produced by sublimation, small portions of air or vapour are caught up, so as to form apparently empty cavities; or, if they are deposited from solution in water, small quantities of water are enclosed, so as to form *fluid-cavities.* In a similar manner, if crystals are formed from a state of igneous fusion, crystallizing out from a fused stone solvent, portions of this fused stone become entangled, which, on cooling, remain in a glassy condition, or become stony, so as to produce what may be called *glass* or *stone cavities.* All these kinds of cavities can readily be seen with suitable magnifying powers, and distinguished from each other by various definite peculiarities.

From these and other facts, the following conclusions were deduced:—

1. Crystals containing only cavities with water were formed from solution.

2. Crystals containing only stone or glass cavities were formed from a state of igneous fusion.

3. Crystals containing both water and stone or glass cavities were formed, under great pressure, by the combined influence of highly-heated water and melted rock.

4. That the relative amount of water present in the cavities may, in some cases, be employed to deduce the temperature at which the crystals were formed, since the accompanying vacuity is due to the contraction of the fluid on cooling.

5. Crystals containing only empty cavities were formed by sublimation, unless the cavities are fluid cavities that have lost their fluid, or are bubbles of gas given off from a substance which was fused.

6. Crystals containing few cavities were formed slowly, in comparison with those of the same material that contain many.

7. Crystals that contain no cavities were formed very slowly, or by the cooliug from fusion of a pure, homogeneous substance.

Applying these general principles to the study of natural crystalline minerals and rocks, it was shown that the fluid cavities in rock-salt,—in the calcareous spar of modern tufaceous deposits, of veins, and of ordinary limestone,—and in the gypsum of gypseous marls, indicate that these minerals were formed by deposition from solution in water at a temperature not materially different from the ordinary. The same conclusions apply to a number of other minerals in veins in various rocks, and to many zeolites. The constituent minerals of mica-schist and the associated rocks contain many fluid-cavities, indicating that they were metamorphosed by the action of heated water, and not by mere dry heat and partial fusion.

The structure of the minerals in erupted lava proves that they were deposited from a mass in the state of igneous fusion, like the crystals in the slags of furnaces; but, in some of these found in blocks ejected from volcanoes (for example, in nepheline and meionite), there are, besides stone and glass-cavities, many containing water, the relative amount of which indicates that they were formed, under great pressure, at a dull red heat, when both liquid water and melted rock were present. The fluid-cavities in these aqueo-igneous minerals very generally contain minute crystals, as if they had been deposited on cooling from solution in the highly heated water. The minerals in trappean rocks have also such a structure as proves them to be of genuine igneous origin, but they have been much altered by the subsequent action of water, and many minerals formed in the minute cavities by deposition from solution in water.

The quartz of quartz-veins has a structure proving that it has been rapidly deposited from solution in water; and, in some instances, the relative amount of water in the fluid cavities indicates that the heat was considerable. In one good case the temperature thus deduced was 165° C. (329° F.); and apparently, when the heat was still greater, mica and tinstone were deposited, and in some cases probably even felspar. There is, then, as has been argued by M. Elie de Beaumont, a gradual passage from quartz-veins to those of granite, and to granite itself; and there is no such distinct line of division between them as might be expected if one was a deposit from water, and the other a rock that had been in such a state of pure igneous fusion as the slags of our furnaces or the erupted lavas. When the constituent minerals of solid granite, far from contact with the stratified rocks, are examined, it is seen that they also contain fluid-cavities. This is especially the case with quartz of coarse-grained, highly quartzose granites, in which there are so many, that the proportion of a thousand millions in a cubic inch is not at all unusual; and the inclosed water constitutes from on eto two per cent. of the volume of the quartz. However, besides these fluid-cavities, the felspar and quartz contain excellent stone-cavities, precisely analogous to those in the crystals of slag, or erupted lavas; and thus the characteristic structure of granite is seen to be the same as that of those minerals formed under aqueo-igneous conditions in the blocks which are ejected from modern volcanoes; and the very common occurrence of minute crystals inside the fluid-cavities still further strengthens this analogy.

The conclusion to which these facts appear to lead is, that granite is not a *simple igneous rock*, like a furnace slag or erupted lava, but is rather an *aqueo-igneous rock*, produced by the combined influence of liquid water and igneous fusion, under similar physical conditions to those existing far below the surface at the base of modern volcanoes.

These deductions of the author, therefore, strongly confirm the views of Scrope, Scheerer, and Elie de Beaumont; and he agrees with them in considering it probable that the presence of the water during the consolidation of the granite was an instrumental, if not the actual cause of the difference between granite and erupted trachytic rocks.—(*Proceedings of the Geological Society.*)

Pliocene Deposits of Montreal.—As I observe in a note in the "Edinburgh New Philosophical Journal" for October 1857, that Professor H. J. Rogers is still disposed to consider the shells found at a height of 470 feet on the Montreal mountain, as having been "swept thither from a much lower level," I presume by earthquake waves; I think it necessary to add to my statements already given, that the shells occur only in stratified sand and fine gravel, alternating in thin layers exactly in the manner of a modern beach. The shells are of course not precisely *in situ*, being arranged in layers among the sand; but their arrangement indicates merely the ordinary action of the waves on the shores of a bay. The error of Professor Rogers may have been caused by his confounding the stratified fossiliferous sand with the unstratified debris which overlies it; and which may perhaps indicate subsidence and ice-drift subsequent to the formation of this beach. The existence of this incoherent terrace of sand and shells perched on a steep and exposed hill-side, is one of the most convincing proofs that could be desired that no cataclysmal waves have swept over the Montreal Mountain since the sea stood at this level. It is proper to add, that Sir C. Lyell, writing in 1845 "Travels in North America," clearly distinguishes the stratified shell-bearing beds from the unstratified mass above.—(J. W. Dawson, *Canad. Nat. and Geol.*, for December 1857, p. 424.)

Vesuvius.—This volcano has been in a state of activity for some time. A letter from M. Verneuil, the well-known geologist, dated 6th January, states that it was then sending forth streams of vapour from two mouths, the one in the centre of the plateau or flattened top of the mountain, the other at the foot of a little cone situated on the east side. The first fumerole is a cavity, and about 164 feet in diameter, and surrounded by three little conical eminences, and the vapour issues from an opening of apparently about 26 feet in diameter. The stream of vapour is continuous, but mingled with violent jets, which are accompanied with discharges of fragments. Looking down from the edge of the cavity, when a violent explosion takes place, red vapours are seen which might be mistaken for waving flames. In 1856 the centre of the plateau was occupied by a circular cavity, 51 feet in depth, from which there were small eruptions at short intervals. This has been filled up and replaced by the small cavity and little cones above mentioned. But another change has taken place which has materially altered the outline of the hill. The eminence at the north side of the plateau, called the *Punta de Palo*, which has towered over the rest of the plateau, we believe for half a century, has disappeared, and the general level is now broken only by the three salient conical eminences, each about fifty feet in height. The breadth of the plateau has not varied much for many years. When the writer of this note visited the hill in 1839, and again in 1847, it was about 2000 feet.—(Charles Maclaren, *in Scotsman.*)

The Tertiary Climate.—Professor Unger of Vienna has found genuine reef forming corallidæ in the Tertiary strata of the Pannonian basin (south-east from Vienna) in latitude 47°, while at present the northern limit of such corals in the Red Sea and Persian Gulf is at 29°, thus furnishing a new proof of the higher temperature which prevailed in Europe at the Tertiary period.—(Charles Maclaren, *in Scotsman.*)

MISCELLANEOUS.

Report of an Expedition undertaken to explore a route by the Rivers Waini, Barama, and Cuyuni, to the Gold-Fields of Caratal, and thence by Upata to the River Orinoco. By W. R. HOLMES and W. H. CAMPBELL, LL.D. (Presented Dec. 7, 1857.)

On the 27th of August last we sailed from the River Demerara, in the colonial revenue schooner Pheasant, and anchored at five o'clock on the following afternoon at the mouth of the Waini, the position of which is laid down by Schomburgk in lat. 8° 25′ N., and long. 59° 35′ W.

From soundings taken on entering the river, and from a subsequent survey by Captain Lyng, we find that the Waini has from 15 to 18 feet water on its bar at spring tides, and would be, consequently, navigable for vessels carrying timber cargoes. On the morning of the 29th August, owing to the heaviness of the rains and fulness of the river, we found that the schooner would not swing to the flood tide: we therefore deemed it necessary to proceed to the River Barama, to procure the assistance of Indians for ascending the Waini—a trip we were not disinclined to undertake, as it enabled us to examine the Mora Creek, a natural navigable canal connecting the mouth of the Waini with the River Barama, some 50 or 60 miles from where the latter falls into the Orinoco. This channel, about 8 miles in length, is of sufficient depth and width, were it cleared of stumps and fallen trees, to enable colony craft to navigate from the one river to the other; and the magnificent timber with which the banks of the Barima and its tributaries abound could thus be easily transported to the mouth of the Waini for shipment. Having obtained a crew of four Warrau Indians, and the spring tides coming on, we were enabled to carry out our preconcerted arrangement for meeting Mr M'Clintock, Superintendent of Rivers and Creeks, at a rocky island about 70 miles up the Waini, or the 4th September. Mr M'Lintock made his way to this point accompanied by about 20 Indians, from the Moruca, through creeks forming an inland navigation of about 100 miles.

On the 6th of September we left the schooner at the mouth of the River Barama, the main tributary of the Waini, and embarked in four canoes with our provisions and "*negotia.*"* Owing to the rapid current of the Barama, and its extraordinary windings, it took us seven days (6th to 12th inclusive) hard paddling to reach the Great Dowaicama Cataract, which has a perpendicular fall of some 30 feet, connected with a series of rapids. Here we had to haul our craft over a portage of about a mile, which caused us considerable delay. On the 15th September we reached an Indian path leading from the Barama to the Cuyuni; but before leaving the former river we think it right to call attention to the inexhaustible stores of the finest timber which cover the banks of the Waini and Barama for upwards of 200 miles, amongst which may be especially mentioned "bullet-tree," "black mora," of enormous size and excellent quality, "silverbally," and "red cedar:" one tree of this description we found floating in the river, the trunk of which, as far as seen, measured 80 feet in length, with a girth of 11 feet 4 inches at 20 feet from the base. Indeed, it may be said that the Waini and its tributaries run through interminable forests of timber, and the same may be stated of the Barama and its tributaries, connected as they are with the Waini by the Mora Creek. Sir R. Schomburgk, after having visited the greater part of British Guiana, reports, "In all my former travels, I have nowhere seen trees so gigantic as on the lands adjoining the Barama in its upper course."

On the 16th September we commenced our march overland to the Cuyuni. Owing to our guides leading us from one Indian settlement to another, we were twelve days reaching that river. We were further delayed by having to carry all our luggage, now reduced to the smallest possible compass, besides provisions for several days for ourselves and the Indians, as we were uncertain of obtaining supplies on the Cuyuni. The paths were tolerably good in most places, and the underwood of the forest was not of the tangled nature of that of the lower or coast regions. The country was undulating, with a constant succession of hill and dale. The hills seldom exceeded from 200 to 300 feet in height, and from the appearance of the soil would be admirably adapted for the cultivation of chocolate, coffee, and other tropical products. Although we were delayed so long in crossing from the Barama to the Cuyuni, we have reason to believe that, were a direct path cut across from river to river, the distance could easily be accomplished in two or three days' moderate walking.

On the afternoon of the 26th September we reached the banks of the

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* Merchandise and trinkets employed in lieu of money, for payment of the Indians.

Cuyuni, a magnificent stream about 500 or 600 yards in breadth even at this distance from its mouth, and some 200 miles from the ocean. Although the river had considerably fallen from its highest level, it still contained a large body of water Its course was generally east and west. Rapids, though not very extensive, were numerous, causing much delay either in hauling over or avoiding them by selecting the smaller and less direct channels, as it almost invariably happened in such localities that the stream of the river was broken by numerous islands. On reaching the Cuyuni we were met by an Accawai Indian, whom we had previously dispatched from our party. He was accompanied by several of the same tribe inhabiting the banks of the river, who furnished us, on reasonable terms, with a small fleet of wood skins, of which we usually had seven. As all our baggage and provisions had to be carried on the overland journey by Indians, we were obliged to leave behind the greater part of the heavier stores, and consequently had to depend on our guns, and the aid of the Indians, for a supply of animal food. We were tolerably successful in obtaining, as we went along, game of various descriptions, and some fish; but our hurried progress did not allow us much time for hunting and fishing. Fortunately our Indians were not difficult to please with regard to animal food, and were satisfied with an ample meal of alligator, guana, or other "bush-meat," not very acceptable to the European palate.

On the 1st of October we passed the mouth of the Curumu, a large tributary which falls into the Cuyuni on the left bank of that river. This stream would have been by far our shortest route to Tupuquen, as it flows from the high savanna lands that extend from about the 60th degree of longitude to the banks of the Orinoco, and runs not far from the village of Tumeremo, which is distant only about 30 miles from the "diggings" at Caratal, but we were unable to take this route, from the quantity of fallen trees which obstructed the channel. If this river proves to be in our territory, British Guiana will possess a large tract of the savanna or table lands so admirably adapted for the pasturage of cattle; and it is to its banks, in our opinion, that any road from this colony should be directed, which would thus at once open to us that immense grazing country whose only outlet for its herds at present is the Orinoco, from which, and from the Essequebo, it is nearly equidistant. On the 30th September we fell in with the first hills approximating to mountains. They gradually developed themselves into the Ekreku range, reaching a height of some 2000 feet, and near the base of which we passed on the 2d October. The scenery here was very striking, the climate genial, the river rapid and sparkling, and its water excellent. The sea or easterly breeze set in about 10 o'clock in the forenoon, and continued to blow all day; the nights were generally calm, but there was a dryness of atmosphere we never experienced in any other part of Guiana; and it was the opinion of our lamented colleague, Dr Blair, that the banks of the upper Cuyuni are well adapted for European settlers. All the way up the Barama, and on our journey across to the Cuyuni, we observed large quantities of quartz, which, with granite and gneiss, formed the principal features of the geological structure of the country. The quantity of quartz gradually increased, and on reaching the Ekreku Creek, which falls into the Cuyuni on the right bank, we found its bottom composed of coarse white quartz sand, or gravel. We could also observe what appeared to be quartz cropping out from the neighbouring hills; and we have reason to think that if we had had sufficient experience and time to make the requisite examination, gold might have been discovered in this neighbourhood.

Having for twelve days worked our way up a part of the River Cuyuni,—so rarely visited by Europeans, and we believe never before described,—on the morning of the 7th of October we reached the River Yuruan, and left the Cuyuni, which was still some 300 yards wide, tending to the south-west; whilst the Yuruan, about 200 yards wide, took a westerly direction. After paddling about eight miles up the Yuruan, we reached the Yuruari, about 150 yards wide at its mouth. The former river continued its westerly course, whilst the Yuruari took nearly a northerly direction. We were here struck by the contrast in the colour of the waters of the two rivers. The stream of the Yuruan was a deep rich brown, and very clear, whilst that of the Yuruari was the colour of milk and water, or white clay, and has the character of being far from wholesome. It contains at any rate a large amount of earthy matter in suspension. At first the stream of the Yuruari was very still and smooth, but ere long we met with a succession of rapids continuing all the way to Tupuquen, exceeding in number and strength of current those of the Cuyuni. Here, too, we were met by a plague of sand-flies that gave us little rest during the day; indeed they seemed most virulent in the hottest sunshine.

On the 9th October we reached the first savanna; it had lately been burned by the Indians for the purpose of securing, as they informed us, the land turtles. It was a grassy wilderness, without a sign of animal life. An Accawai Indian had built his house on the top of a hill commanding a fine and extensive view, and thousands of acres of pasture land were lying before us totally unoccupied. For many miles the Yuruari was bounded on either side by savanna land, with a narrow strip of bush lining its banks. As we approached Tupuquen we fell in occasionally with cattle farms, most of which had formerly belonged to the late Colonel Hamilton, who owned a vast tract in this neighbourhood. In some instances the proprietors resided on their own lands, in others the farms were managed by *major domos.* The number of cattle said to be on each farm was very large—from 10,000 to 20,000—but that anything like this number would be actually available, we are not at all prepared to assert.

On the 13th October, about mid-day, we reached the landing-place of Tupuquen, the village being situated half a mile from the river. We were here met by a Mr Gray, son of a former cattle proprietor of this colony, who conducted us to Tupuquen, and introduced us to the Alcalde. The village of Tupuquen consists of some 50 or 60 mud tenements covered with tiles, hardly worthy of the name of houses. It formerly constituted one of the 32 missions into which this part of the country was divided under the old Spanish régime, and each of which was presided over by a Capuchin friar. The revolution upset this order of things, and although the houses nominally belong to the Indians, they are now mostly appropriated by other occupants, whom the attractions of the "diggings" have drawn to this otherwise out-of-the-way spot.

On the morning of the 14th October we started for the "diggings" at Caratal. After crossing the Yuruari, a sharp walk of about two hours through the forest, over hill and dale, brought us to a village consisting of about fifty thatched logies, varying in size from a mere hut to that of an ordinary house. As these buildings were mostly without walls, and open all round, it speaks well for the honesty of the diggers that thefts were almost unknown. The number of people congregated here it was difficult to ascertain; they were variously estimated at from about 120 to 200, the latter number being the outside. The "diggings" are situated in the primitive forest, and consist of a number of holes or pits dug by individuals or small companies. There are no stringent laws for the regulation of this community. Each individual is at liberty to select any unappropriated spot, and to commence operations. There are few external indications of the probability of success, except that gold had been dug out in the immediate vicinity, and even this was not a safe criterion, as, although the precious metal might be found in one hole, the adjoining spots, although almost touching, often prove blanks.

The method of proceeding is as follows:—A piece of ground, say 8 feet by 20, having been selected, the miner in the first instance has to clear off the bush, and generally to dig out a forest tree of considerable size. Having removed the upper soil, he arrives at a harder subsoil, which has to be loosened with a pickaxe previous to being shovelled out. At times, after getting down some seven or eight feet, water takes possession of his pit, at others he meets with solid rock; in either case his labour has been thrown away; but if his speculation has a more fortunate aspect, he falls in, at an average depth of from ten to fifteen feet, with what is technically called the "Graja," or a layer of earth, clay, quartz, and ironstone, in which stratum, overlying stiff clay, the gold is found. The whole of this layer, generally about a foot in thickness, must be carefully thrown out on the bank, and having been collected in a mass, has to be taken in sacks on the back about a quarter of a mile to the nearest water, there to be washed, parcel after parcel, in a cradle, to do which the miner must sit up to his middle in water. If fortune favours him, after washing away a cradleful of soil, he may find some particles or small nuggets of gold; but frequently it is all in vain, and cradleful after cradleful disappears without a sign of the precious metal. It is hard to say how long it would take an individual to go through the whole operation of clearing, digging, and washing; but, on an average, it would require three weeks' hard labour, and it is still more difficult to say what the result would be. The proportions are about six blanks to one substantial prize. There is, however, no denying that at times the reward is great. If Caratal were a healthy place, perhaps the chances of success might be a sufficient inducement for an industrious, persevering man to try his fortune; but endemic disease prevails to a great extent. We did not meet a single individual who had not suffered more or less from fever, and many from "Béche," or inflammation of the lower bowels, which is supposed by the miners to be induced by the inferior quality of the only water in the neighbourhood. There is no medical advice to be had even at Tupuquen; and medicine, if procurable at all, was only so at exorbitant prices—a bit, or 4d. a grain being the retail price of quinine of dubious quality. The only food obtainable is beef and cassava bread; and as meat keeps so short a time, fresh beef was often not to be had. The usual food of the miners is tasso, or beef dried in the sun, a most unpalatable and unwholesome article of diet; for, as little salt (an expensive condiment in Venezuela) is used, the meat is generally much tainted. On the whole, taking into consideration the labour, the sickness, the want of medicine and medical advice, the insufficient and innutritious food, the vermin (the "diggings" abound in fleas, chigoes, bête rouge, ticks, and ground itch), and the total absence of ordinary comforts, we do not hesitate to say it would be an act of folly in an industrious man to leave this colony for the "diggings," even were success in the search for gold much more certain than it is. But how much more is it to be deprecated, when, from all we could learn, and we took considerable pains to ascertain, we really believe a larger average day's wages could be earned on any estate in the colony than could, under the present circumstances, be made by digging at Caratal. We met several individuals, natives of this colony and of the British West India Islands, who bitterly lamented having left their homes. Some of these, with shattered health, and in debt, were unable to undertake a journey requiring twelve or fifteen days' walking to reach Las Tablas, the port of embarkation on the river Orinoco, and seemed to be hopelessly awaiting their fate at Caratal.

As our journey to Caratal had taken a much longer time than we anticipated, we were most anxious to push on so as to hasten our return to Georgetown. We therefore remained only two days at the diggings, and although our Indians, under the superintendence of Mr M'Clintock, had commenced to dig a "barranca," as the pits are technically called, we did not await the result, and left when they had got down about eight or ten feet. Before leaving we picked up several specimens of quartz rock containing particles of gold, also a few pieces of the same rock with small particles of a white metal, supposed to be platina. On the 18th October, having hired three horses for ourselves, wretched animals, for which we were charged an exorbitant price, four donkeys for baggage and servants, and a mounted guide, we started for the town of Upata. In crossing the wide savanna it would have been impossible to have proceeded without a guide, as our path was marked almost solely by the numerous cattle-tracks that crossed it in every direction. We were much struck with the park-like scenery of the country; hills at least 1500 feet high, covered with verdure to the very tops,—here and there clumps of trees dotted the plain, whose well-defined back ground consisted of more extensive woods: such was the landscape all the way to the village of Guacipata, where on 19th October we put up for the night. We were most hospitably received by the wife of Senhor Miranda, who, in the absence of her husband, offered us the best her house afforded, and declined any remuneration. It may not be amiss here to state, that in a country where inns are unknown, and where travellers are consequently thrown on the kindness of the inhabitants, we were most cordially received; and although their ordinary food, tasso and cassava bread, was most unpalatable to us, it was produced with good will, and in abundance, for ourselves and servants. It is strange that in a cattle country, such as we were now traversing, milk and cheese are but rarely used, the former, as they allege, predisposing to fever, while butter is unknown. The village of Guacipata is another of the missions previously alluded to. Here the church, a building 150 feet in length by 50 in width, is in tolerable preservation; the dwellings of the monks also still remain.

From Guacipata it took us four days' slow riding, as we had to wait for the donkeys and baggage, to reach Upata. The scenery continued much the same, hill and dale, verdure and wood, with mountains in the distance. On one side was the continuation of the Caratal hills, on the other the Nuria range extending for many miles. They all bespoke, from their formation and appearance, volcanic agency. The whole country abounds in quartz. The sides of the hills were frequently covered with masses of this rock, of the purest white, which reminded us of Sir Walter Raleigh's description, where he characterizes them as blocks of "white spar, *el madre del oro.*" At a distance they often resembled the groupings of large flocks of sheep. The whole region is one continued tract of pasture land, with frequent water courses, and although the herbage is rather coarse, it is apparently admirably adapted for cattle. The number we saw was comparatively small, but they all looked sleek, and if not fat, appeared thriving and healthy. We passed about half a dozen farm-houses on our route, and either breakfasted or slept at several of them. The "Hacienda," or farm called Para Para, struck us as a favourable specimen; besides some 20,000 or 30,000 head of cattle, and 100 horses, the owner had several acres of sugar cane and tobacco under cultivation. His house, built of mud, as all the houses are in this province, was extensive. The offices included a sugar-mill and boiling-house. The mill consisted of three upright wooden rollers propelled by animal power, he had two teaches in his copper wall, and made his 300 "papillons" or loaves of brown sugar in a day. The sugar is run into moulds cut in a block of wood, and by boiling high he manages to set the whole of his liquor, which is thus concentrated in the papillon, leaving no residuum. His tobacco crop had just been reaped; it seemed of good quality, and must be in much demand, as men, women, and children are all inveterate smokers in Venezuela. He valued his cattle all round at about 10 "pesos" or 8 dollars per head, and this price would include the farm-house and all its adjuncts. The tenure of land in this part of Venezuela is rarely freehold; any person can apply for unoccupied land, and, on its being surveyed, can obtain a license of occupancy on the payment of a small sum annually to Government; he then proceeds to stock it with cattle, or to cultivate it; and, except in the event of a revolution, is rarely disturbed in any way.

On the 22d October we reached the town of Upata, which has the most thriving appearance of any place we saw on our route. It is composed of about a dozen streets of one-storey tiled houses; but there is an air of prosperity about it, arising chiefly from the traffic which has sprung up with the diggings, all property and merchandise destined for Tupuquen passing through Upata. Here, for the first time, we came in contact with the local authorities; a gentleman from the municipality required to see our papers, and although we demonstrated to him that everything was according to rule, he appeared hardly satisfied. On the 22d October we left Upata and breakfasted with Senhor Pedro Maria Nunes. In the forenoon we explored, under the guidance of Mr Dassger of Upata, a hill opposite to Senhor Nunes' residence, apparently composed of a mass of what seemed to be almost pure iron.* The country between Upata and Las Tablas is very mountainous and woody, and the grazing lands are more contracted. We however passed several cattle farms. On the 3d day, October 5, we reached Las Tablas, a village situated on the river Orinoco, of some 40 or 50 mud houses, partly tiled and partly thatched. We were most hospitably received by Mr Behrens, whose house is the principal mercantile firm of the province. The distance by the route we travelled from Tupuquen to Las Tablas we estimated at 150 miles. There is a somewhat shorter road by Pastora. Las Tablas is not a port of entry, and derives its importance chiefly as the place whence a large number of cattle are shipped; and, being the nearest point on the river Orinoco to Upata, all merchandise passes through it on its way to that town.

On the 26th October we started in a hired corial for Barancas, and reached that place after twelve hours' hard pulling in an open boat. We remained there one day (27th), having been kindly offered by Mr Burnett a passage in the "Loyal" (a cattle vessel trading to Cayenne), to Point Barama, where the "Pheasant" was waiting for us. Barancas is situated on the left bank of the Orinoco, and the river falls off here nearly forty feet between the months of July and December. The town is suronnded by lagoons connected with the river; these were in the process of drying up, and the inhabitants were suffering much from fever. As up to this point not one of the Expedition had suffered from a day's ill health, it must be inferred that here were sown the seeds of that fever which in the case of our lamented colleague, Dr Blair, terminated so fatally. Both ourselves and servants were laid up with fever, and suffered much during a three days' paasage down the Orinoco from Barancas to the mouth of the River Barama. Dr Blair alone had so far escaped. On the morning of the 31st October we reached the "Pheasant," lying off Point Barama, and we all felt the greatest satisfaction in joining her. In the evening we took advantage of the ebb tide to get under weigh, and next morning we were off the mouth off the Waini. About ten o'clock Dr Blair was seized with what he considered congestion of the lungs, and bled himself; he had two relapses; on each occasion he reopened the vein; extreme exhaustion came on, and continued until our arrival in Georgetown at four A.M., on the 5th November, after an absence of exactly ten weeks. The fatal termination of his illness on the 9th is too recent and too melancholy to require further notice or comment in this place. Here, however, we may be allowed to remark, that our late colleague was the soul of the Expedition; his fine clear intellect, his analyzing and observant mind, enabled him to arrive at rapid as well as just conclusions. He was struck with the great natural capabilities of the upper Cuyuni, with its beautiful scenery and genial climate, and although we were ten days paddling up that river, so varied was the landscape, that each day's journey was looked forward to with pleasure. At the diggings at Caratal, Dr Blair was beset by invalids for advice, which he invariably and gratuitously afforded. Our small stock of medicines rapidly disappeared, and it is even possible that if he had not given away the last dose of quinine to a sufferer at Barancas his own most valuable life might have been saved.

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* A specimen having been submitted to Dr Shier for examination, he has ascertained it to be "Brown Hematite," a very rich iron ore. Mr Draeger informed us that immense quantities of this ore are to be found on the mountains for thirty or forty miles on each side of Upata.

We cannot conclude this Report without alluding to the Indian population inhabiting the country between the rivers Pomeroon and Amacuru, the Atlantic Ocean and the river Cuyuni. Mr M'Clintock, a good authority, as he made a census of the Indian population some years back, estimates their number at about 2500. During our expedition we at various times had with us from thirty to forty Indians of five different tribes. We found them invariably truthful and honest; during eight weeks that they were with us we never missed the most trifling article. They were assiduous and willing, easily satisfied as to food, and we have much pleasure in recording our unqualified satisfaction with their conduct. It must not, however, be forgotten that they were under the supervision of Mr M'Clintock, who for many years has been Superintendent of Rivers and Creeks for the Pomeroon and adjacent districts. The unbounded confidence which the Indian population repose in this gentleman speaks well for both parties, and no donbt they have acquired this feeling by many years' experience, during which Mr M'Clintock has been their protector.

We may add, that in the event of a road being cut, as previously suggested, to the savanna land in the neighbourhood of the Curumu, a large band of labourers, skilful in bushwork, could easily be collected from the Indian tribes in the Waini and its tributaries, peculiarly qualified for that description of employment.

The Poison of Upas Antiar (Antiaris toxicaria). By Professor KÖLLIKER of Wurzburg.—1. The Antiar is a paralyzing poison. 2. It acts, in the first instance, and with great rapidity (in five to ten minutes) upon the heart, and stops its action. 3. The consequences of this paralysis of the heart are the cessation of the voluntary reflex movements in the first and second hour after the introduction of the poison. 4. The Antiar paralyzes, in the second place, the voluntary muscles. 5. In the third place, it causes the loss of excitability of the great nervous trunks. 6. The heart and muscles of frogs poisoned with Urari may be paralyzed by Antiar. 7. From all this is may be deduced, that the Antiar principally acts upon the muscular fibre, and causes paralysis of it.—(*Proc. Royal Soc. Lond.* Dec. 1857.)

Magnetism.—From a table published by Encke, in the Memoirs of the Berlin Academy, it appears that in the 15 years between 1839 and 1854 the magnetic "declination," or the westerly deviation of the magnetic north from the true north, has diminished 1° 49½′; the "variation" has, therefore been at the mean rate of 7⅓ minutes per annum; but it has been a little greater in the second half of the term than the first. The declination at Berlin in 1854 was 14° 56′ 52″.—(Charles Maclaren, *in Scotsman.*)

ABSTRACT OF THE METEOROLOGICAL REGISTER FOR 1857,

Kept at Arbroath, by ALEXANDER BROWN, Honorary Member of the Literary and Philosophical Society, St Andrews; Observing Member of the Scottish Meteorological Society, &c.

Latitude 56° 34′ N. Longitude 2° 35′ W. Distance from the Sea, ⅜ths of a Mile.
Height of the Barometer above the Sea, 80 feet; height of the Thermometer from the ground, 11 feet, and of the Rain-Gauge, 2 feet.
The number of "Rainy Days" includes those days on which snow or hail fell.

1857.	BAROMETER.		THERMOMETER.							Days Ther. below 32°.	Rain in Inches.	HYGROMETER.		Fair Days.	Rainy Days.	WINDS, AT 8¾ A.M.									
	Corrected to 32°, and reduced to sea-level.		8¾ A.M.	7¾ P.M.	Mean.	Mean Max.	Mean Min.	Mean.	Spring Water.			Mean Dew Point.*	Degree of Humidity (complete Sat. 1·000).			N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	Total.
	8¾h. A.M.	7¾h. P.M.																							
January . .	29·65	29·64	38°·1	33°·8	33°·4	38°·3	29°·8	34°·0	47°·8	24	2·012	30°·2	0·906	19	12	1	1	3	1	—	4	11	8	2	31
February .	29·86	29·86	37·1	38·1	37·6	43·9	31·8	37·8	47·0	15	1·116	34·0	0·900	18	10	1	—	—	1	5	7	8	2	4	28
March . .	29·80	29·77	38·9	39·6	39·3	44·9	35·1	40·0	46·5	8	2·696	34·0	0·840	18	13	1	2	9	3	2	2	9	2	1	31
April . .	29·76	29·75	44·2	43·4	43·8	49·8	38·0	43·9	46·5	2	3·266	39·4	·847	11	19	2	2	11	4	2	1	3	1	4	30
May . .	29·97	29·97	51·3	50·2	50·7	57·1	43·1	50·1	46·8	—	2·188	44·5	·787	18	13	5	2	11	2	2	3	1	—	3	31
June . .	30·02	30·01	59·3	57·8	58·5	65·0	48·5	56·7	48·0	—	2·666	52·8	·804	22	8	3	3	7	6	—	2	3	2	4	30
July . .	29·83	29·83	61·7	61·3	61·5	68·9	51·4	60·1	48·5	—	0·961	53·7	·765	23	8	2	—	1	1	5	8	8	6	—	31
August .	30·01	30·00	61·1	59·5	60·3	67·5	53·5	60·3	49·5	—	1·633	54·5	·820	21	10	5	2	3	5	6	1	4	1	4	31
September .	29·87	29·87	57·5	55·8	56·6	63·8	51·1	57·5	50·0	—	2·568	52·7	·854	20	10	3	—	2	1	3	5	5	3	8	30
October .	29·79	29·77	48·5	49·2	48·8	55·7	44·0	49·8	50·0	1	1·279	44·3	·867	19	12	4	1	2	5	1	6	7	2	3	31
November .	30·08	30·08	42·7	43·3	43·0	48·3	39·3	43·8	50·0	7	2·397	40·3	·925	21	9	4	2	5	1	—	5	1	3	4	30
December .	29·95	29·94	43·2	43·8	43·5	47·5	39·2	43·3	49·0	2	1·012	39.0	·897	24	7	—	—	—	1	3	8	16	1	2	31
Mean . .	29·80	29·79	48·2	47·9	48·1	54·2	42·0	48·1	48·2	59	23·739	43·2	0·851	234	131	31	17	54	31	29	52	76	36	39	365
Do. 1856 .	29·84	29·83	44·9	44·5	44·7	51·8	38·2	45·0	47·5	94	32·274	39·3	0·825	214	152	53	18	44	35	23	22	72	69	30	366

Barometer at 8¾ A.M. was highest on 11th November, 30·73.† Wind N.W.
Do. do. was lowest on 14th March, 28·67. Wind S.E.
Do. at 7¾ P.M. was highest on 11th November, 30·72. Wind N.W.
Do. do. was lowest on 14th March, 28·50. Wind S.E.
Thermometer at 8¾ A.M. was highest on 28th June and 3d August, 70°. Wind E. and W.
Do. do. was lowest on 29th Jan. and 4th Feb., 24°. Wind N.W. & calm.
Do. at 7¾ P.M. was highest on 26th June, 71°. Wind S.W.
Do do. was lowest on 28th January, 22°. Wind N.W.
Thermometer in night, highest, 20th August, 63°; lowest, 29th January, 21°.
Thermometer in day, highest, 26th June, 81°; lowest, 29th January, 27°.
Coldest day, 29th January, when average of Thermometer was 24° for day and night.
Hottest day, 26th June, when average of Thermometer was 67½° for day and night.
Coldest month of the year, January; hottest, August.
Wettest month of the year, April; driest, July.
Mean temperature of the year, 48·1 degrees.
Mean temperature of fourteen years, 45·715 degrees.

For eleven years the average daily temperature at 8¾ A.M., at 7¾ P.M., and the mean of the daily extremes, are as follows, viz.:—

	8¾ A.M.	7¾ P.M.	Mean of Extremes.
1847	45°·6	44°·7	45°·9
1848	45·1	44·4	45·0
1849	45·1	44·4	44·7
1850	45·5	44·9	45·5
1851	45·7	45·4	46·3
1852	46·0	45·9	46·2
1853	45·4	44·9	45·6
1854	46·3	46·4	46·3
1855	44·2	43·9	44·3
1856	44·9	44·5	45·0
1857	48·2	47·9	48·1

The average temperature of the six months of chief vegetation, viz., those from April to September inclusive—for ten years, is as follows:—

1848 ...	51°·7	1850 ...	51°·7	1852 ...	53°·0	1854 ...	53°·5	1856 ...	50°·9
1849 ...	52·2	1851 ...	52·0	1853 ...	53.4	1855 ...	52·1	1857 ...	54·7

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* The dew-point thus found is obtained from observations of the Wet and Dry Bulb Thermometers, and deduced by Mr Glaisher's Hygrometrical Tables. The observations of the Wet and Dry Bulb Thermometers are made daily at 8¾ A.M. The times of observation of the instruments stated in the Table are Greenwich mean times.

† The highest and lowest readings of the Barometer are reduced to sea-level, and otherwise corrected.

Astronomy—Academy of Sciences, February 8, 1858.—The Lalande prize this year was divided between Messrs Goldschmidt of Paris and Bruhns of Berlin. The former has added four to the number of small planets; the latter discovered the comet of March 1857, which has since been identified with that discovered on 26th February 1846 by M. Brorsen, and which was looked for in 1851, but not seen. Its period must be about 11 years. At the meeting on 15th February, M. Leverrier announced that another comet had been placed on the periodical list. Mr Maclear, at the Cape of Good Hope, had succeeded in finding the comet discovered by M. D'Arrest in 1851. Its periodicity had been recognised by M. Yvon Villarceau, who had computed its ephemerides, by means of which Mr Maclear, knowing exactly the position it ought to occupy, was able to find it on its return, in spite of the feebleness of its light. Its period must be about six years.

The 52d of the group of small planets was discovered by M. Goldschmidt on the 4th of February, in the constellation of the Lion. It is a star of the tenth magnitude.—(Charles Maclaren, *in Scotsman.*)

OBITUARIES.

Obituaries. By Dr Christison, in his address to the Royal Society.

1. William Henry Playfair, a Scotchman by descent, and a citizen of Edinburgh from his youth, was born in London, where his father, the brother of our former Professor, Philosopher, and Secretary, John Playfair, practised as an architect of repute. Educated here under the eye of his uncle, and living much in the society of a host of his uncle's pupils, comprising a multitude of young men of talent, who have since risen to great eminence in many departments of human knowledge, Mr Playfair acquired an extensive acquaintance with Learning, Science, and Art, and above all, in his own profession, a correct and fastidious taste, of which we now reap the fruits in this city.

At the early age of twenty-six Mr Playfair was chosen by his Majesty George the Third's Commissioners to carry out the erection of the buildings of the University,—his first great work, in which he was at one and the same time aided by the general grandeur, and cramped by the faulty details of his precursor Adam,—and in which he ultimately triumphed over every difficulty.—There is nothing in our northern metropolis to compare with the simple stateliness and chaste details of the interior quadrangle of the University,—which is mainly Playfair's own,—for his predecessor contemplated the monstrous and fatal blot of a double quadrangle, with differently elevated courts;—and we have nowhere else any single apartment that combines so chastely and harmoniously the vastness of space, architectural splendour, and bibliothecal fitness of the upper Library Hall.

It would be out of place for me to notice here all Playfair's public works, which have been principally erected in Edinburgh, and constitute a large proportion of the most conspicuous architectural decorations of the city, and bid fair to immortalize him, so long as the capital of Scotland shall continue to attract, as it does now, visitors of taste from all quarters of Europe and America. Among critics in architecture it may be wished that some of them were better. But was it the architect's fault that they are not so? In every one of his works, except Donaldson's

Hospital, he had to encounter great difficulties of site, or neighbourhood, or both together,—difficulties, indeed, sometimes unconquerable by any skill. And yet even in these, when he is said to have failed, the critics who think so appear to me to proceed for the most part upon the assumption, that he had within his choice plans of far greater magnitude than his limits, and a command of means far beyond his actual treasury. Who, for instance, can say what might not have been the felicity of an architect, so pure in his style, and so fruitful in his resources, had he been told when he designed the columned temple in a portion of which our Society is now accommodated, that he was afterwards to cover the Mound, from the bottom to the crown of its slope, with public edifices? —and that he was at liberty to do so at a cost of twice, thrice, or four times the L.100,000 which have been actually expended on them?—for that seems conditional to the criticisms to which one often hears Playfair subjected, on account of his designs for the Royal Institution's Building, the National Gallery, and the Free Church College.

Of all his works none has called forth such unqualified applause as Donaldson's Hospital; and his success there was all the more remarkable, because the style was altogether new to him. This has been described by one of his most successful ephemeral biographers,—plainly a zealous, yet impartial and able admirer,—as a type of Gothic style; for which the author is obliged to admit, with evident compunction, the unhappy cognomen of "Debased Gothic." But let us call this work of Playfair's hands more fitly the "Inhabitable Gothic;" and no one has been more perfectly successful in making the Gothic habitable than our deceased fellow-member. No pleasure however is without alloy. There are few who will not regret that so magnificent a pile had not been destined for a more conformable object. Scotchmen were usually charged in former days by their neighbours with presenting, by a species of elective attraction, the frequent union of poverty and pride. It may be allowable in a native of the Scotch metropolis to lament that the old sneer should be verified in these present times by the pride of lodging poverty in such a palace.

I am assured that Playfair was so conscientiously fastidious in discharging the trust reposed in him as a professional man, that he executed all his drawings with his own hands. When engaged in this task, he for many years constantly worked in the standing posture, often for twelve hours a-day. To this habit he himself ascribed, not without justice, a paralytic affection of the spine, which gradually stole upon him when he was a man of middle life only. Slowly increasing year after year, it at last prevented in a great measure locomotion. But his aptitude for exercise of the mind continued unimpaired long afterwards. And even when his sad malady, spreading upwards, enfeebled his arms, and at length invaded also his mental faculties, it only required a new point in his plans to need consideration, when he was aroused to his old perspicuity and decision, and the point was settled.

Playfair was, in every good sense of the words, a scholar and a gentleman. As such his society was courted on all hands. But for many years his infirmities had withdrawn him very much from the social circle; so that few except one or two old intimates can now tell how much society has lost in this respect by his death. He died in his sixty-eighth year. No one can doubt that his memory will long survive in his works.

2. The biography of WILLIAM SCORESBY belongs not so much to us as to the parent Society of the sister kingdom. But as this remarkable man frequently visited us, joined us as an Ordinary Fellow, sometimes contributed to the business of our meetings, and was in early life a student of our University during the winter intervals of repose from his voyages of arctic adventure, it becomes me to advert shortly to the departure of one so eminent in science, so amiable in disposition, so distinguished for Christian virtue.

Scoresby was the son of an experienced whaler and able navigator of Whitby, in Yorkshire. The father's zeal in his profession was so intense and catholic, that he actually carried off his child to his favourite arctic regions at the age of ten, without the previous knowledge of Mrs Scoresby,—an attached wife, and no less fond a mother. The idea, it must be added, did not occur to the father till he one day detected, with much trouble, the urchin hidden below in the Resolution, while the "Blue Peter" was flying from the mast-head, and when the boy had a clear intention of running away from home in this remarkable manner. Entering thus early on a life of fearless adventure, it is no wonder that the second Scoresby outstripped the first in eminence as a navigator. At the age of sixteen, he discovered with his father an open sea near Spitzbergen, apparently stretching towards the North Pole; and he actually sailed in it to the latitude of 78° 46′,—the highest known to have been ever attained up to that time. At the age of twenty-one he succeeded his father as commander of the Resolution whaler of Whitby; and for twelve years afterwards he annually fished the Greenland Seas, carrying on at the same time constant researches in geography, magnetism, geology, and zoology; for which he had prepared himself by several winters of study under Jameson and other Professors of the University of Edinburgh. The results were published in his "Account of the Arctic Regions," and in his "Voyage to the Northern Whale Fishery."

A deep, pure vein of piety, fostered by careful early training on the part of his parents, everywhere pervaded his pursuits, whether professional or scientific. No whale was hunted, and no other work that could be dispensed with was done by the crew of the Resolution on the Sabbath. Their captain was constantly as assiduous in maintaining the religious condition of his men as in preserving their health, and availing himself of their seamanship. But it is also recorded of him, that he generally contrived to reward the forbearance of his men while their game was sporting securely on all sides around them on Sunday, by insuring that they should make prize of a whale or two at the first entrance of the hours upon Monday morning.

The depth and sincerity of his feelings as a responsible creature he has recorded in his "Sabbaths in the Arctic Regions." The ultimate consequence of his following this bent of his mind was, that, while still in the prime of life and vigour, he deserted his favourite the sea, studied at Cambridge for the English Church, took, soon afterwards, the degree of Doctor of Divinity, and became a zealous and efficient member of the ministry, first among his co-mates as chaplain to the Mariner's Church at Liverpool, and eventually at Bradford, as pastor of an extensive manufacturing population.

The ardent and conscientious discharge of his religious duties, however, did not prevent him from applying also to the favourite scientific pursuits of his youth. Only a year before his death, indeed, he undertook a voyage to Australia, for the purpose of testing his theory respecting the aberration of the compass in iron ships; and one of his last scientific observations was the measurement of the ocean wave in a storm off the Cape of Good Hope, when he ascertained that the elevation of the highest, when the sea "ran mountains high," was forty feet from trough to crest.

I cannot, consistently with the indispensable brevity of this sketch, even so much as enumerate Dr Scoresby's many contributions to science; but must hasten at once to the close of this theme. Scoresby died, after a tedious illness, at a fair old age, in his sixty-eighth year. Few men can at that age console themselves with the retrospect of so long an existence so usefully spent. The intrepid seaman, the skilful navigator, the philosopher of no mean order, and the pious divine, was throughout his entire life full of good works in each and all of his multifarious vocations.

3. The connection of Marshall Hall with our Society has been somewhat similar to that of the arctic navigator. Born in Nottinghamshire, and trained there till his nineteenth year, he then came to this city in 1809 to pursue the study of medicine. He graduated at our University in 1812; remained two years longer as one of the resident physicians of the Royal Infirmary; was elected during that period President of the Royal Medical Society, an office which has generally been the forerunner and presage of future distinction; delivered, it seems, a short course of lectures on the Diagnosis of Diseases, ever afterwards a favourite subject of inquiry with him; and on leaving this, to settle as a physician in Nottingham, continued to maintain his predilection for Edinburgh, as is shown by his having joined its Royal Society as a Fellow in 1819. But this has been the full amount of his connection with us.

He had been scarcely twelve years in Nottingham, when the promptings of genius induced him to seek a fitter field for its development in London, where he slowly attained a respectable place as a physician. His contributions to the practice of his profession, both before and after he settled in London, were numerous, always ingenious, often original, generally valuable, but sometimes controvertible. Of all these contributions, none perhaps will convey a higher idea of his acute and inventive discrimination as a physician than his inquiry, begun in 1824, and perfected some years afterwards, into the constitutional effects of the loss of blood, of which he successfully investigated the phenomena, supplied the explanation, and detailed the conclusions, in the shape of valuable instruction, for distinguishing between inflammation and nervous irritation, thereby laying down the means of escape from fearful errors at that time often committed by the incautious and uncompromising admirers of blood-letting as a remedy.

But the credit which may be justly claimed for Marshall Hall for his contributions to medical experience and practice sinks into insignificance when compared with his higher fame as a physiologist. It belongs properly to the sister Royal Society to sketch biographically the details of his discoveries in physiology. From me they can receive but a brief and passing notice, without too great a demand on your time and attention. I must confine myself, indeed, to only one of them, but that the greatest of all, the precursor and foundation of all the rest, and sufficient of itself to stamp Marshall Hall as an inventive genius, whose name will go down to posterity as one of the pillars of physiological science in the present century.

It is evident from his works that Marshall Hall's attention had been eagerly turned to the immortal discoveries of our greatest Scottish physiologist in these recent times, the late Sir Charles Bell, in regard to the functions of the brain, spinal marrow, and nerves. From that moment the nervous system was his great centre of attraction. Sir Charles first sighted, and laid down in an undeniable shape, the grand fact in the physiology of the nervous system, that sensation is conveyed, and motion governed, by different nerves, or different filaments of nerves, having different origins in the cerebro-spinal system. Hall, however, was the first to see that this separation of what were once conceived to be common functions of all, or almost all, nerves, was not enough to account for the whole phenomena of nervous action. He showed that, sensation being conveyed from the circumference to the centre, the brain, by one set of nerves, or filaments of nerves,—as Sir Charles first indicated,—and motion being excited by volition sending an influence from the centre to the circumference by means of other nerves or nervous filaments,—also a branch of Bell's discoveries,—there is another class of actions caused, independently of volition or of consciousness, by external impressions made directly on the spinal marrow itself; and, above all, that there is another set of numberless mysterious movements and actions, mysterious formerly,—but intelligible and clear as noon-day since his inquiries have been accepted,—which are excited by an agency, conveyed first from the circumference along *afferent* filaments of nerves to the spinal marrow as their centre, and thence along other or *efferent* nervous filaments to the circumference where action is eventually manifested, and all this independently of volition, often too of sensation, and not unfrequently of consciousness. These actions, which are constantly illustrated in the exercise of our functions, such as in the acts of breathing, swallowing, discharging the excretions, sneezing, coughing, winking, and the like, constitute what are called by Hall *Reflex Actions*. They are also exemplified by a thousand phenomena occurring during disease. Let me instance one example, which will at once render his discovery of reflex actions intelligible to any common understanding. When, in poisoning with prussic acid, the sufferer is perfectly insensible and motionless, and no muscular action is discoverable except a spasmodic upturning of the eyeballs, and a slow, short, imperfect respiration,—if we pour upon the head suddenly a full stream of cold water, instantly a deep inspiration is drawn, which fills the whole chest. By repeating this process, we remove several of the immediate and sure causes of death, and may restore consciousness, sensibility, and at last perfect health. But this by-the-by; the main purpose in quoting the fact now is to exemplify an action caused by an impression on a part of the nervous circumference, conveyed by certain nervous filaments to the spinal marrow, and transmitted instantly by certain other nervous filaments to the muscles which maintain respiration,—and quite independently of volition, of sensation, of consciousness; of all the cerebral functions, in short, which, in the case supposed, are totally dormant and suspended. This is a reflex action, one of a countless multitude of phenomena which were entirely, or almost altogether, misunderstood, until Marshall Hall caught the first glimpse of them, investigated, elucidated, and classified them, and deduced innumerable conclusions from them for explaining previously incomprehensible phenomena occurring in health, and still more in disease. This is the grand fact, the discovery of which we owe to Marshall Hall, and from which he afterwards proceeded to further discoveries in the physiology of the nervous system.

Like other discoverers, he at first encountered much opposition to his new views. But all physiologists and physicians are now agreed in adopting the most important of them, and in acknowledging the obligations which physiology and medical practice owe to him. For many of the latter years of his life he was esteemed as one of the most successful physiological inquirers in Europe. He persevered in his researches till near the end of his life, which terminated in a slow and painful illness before the close of his 67th year.

Obituary of M. Thénard. By Professor Kelland.—For the information I have acquired relative to this excellent chemist, I am indebted to Dr Christison, who has furnished me with his personal recollections, and with a biographical souvenir of the deceased by one of his former assistants, M. Le Canu.

The association of the name of Thénard with the progress of chemistry dates back to the period of history. His first contribution to the science was made so early as the year 1799; the subject being "The Oxygenated Compounds of Antimony, and their Combinations with Sulphuretted Hydrogen." His last was presented in 1856, fifty-seven years later, and is entitled "Memoir on the Bodies whose Decomposition is effected under the influence of the Catalytic Force." To detail all the discoveries of an author whose writings are scattered over so vast a period would be a work of some labour, and might justly be regarded by many of my hearers as a dry and unnecessary detail. A few of the more important only can be noticed.

We owe to him the production of muriatic ether. It is true, however, that Boullay in France, and Gehlen in Germany, made the discovery about the same time with himself. We owe to him also the discovery of oxygenated water, or the binoxide of hydrogen, and consequently that of the peroxide of calcium of copper, &c., which it produces by reacting on the inferior oxide sof these metals. M. Le Canu admits, in reference to this discovery, that a happy accident exhibited to M. Thénard the dissolution of binoxide of barium in water acidulated with nitric acid, without the disengagement of oxygen; but he argues very justly that the merit consisted in the far-seeing power which could divine the existence of a definite combination of oxygen and hydrogen, essentially distinct from ordinary water.

M. Thénard had the good fortune to labour in conjunction with a host of great men—with Fourcroy, with Dulong, with Biot, with Dupuytren, but, above all, with Gay-Lussac. It is in this last connection, I imagine, that his name comes most frequently under the eye of non-chemical readers amongst us. Gay-Lussac and Thénard published, in conjunction, a series of most valuable memoirs, which were afterwards united in two volumes. Of these volumes Berthollet thus speaks: "They seem to constitute a new science, raised on the old sciences of physics and chemistry as their groundwork." Amongst the vast mass of discoveries which these researches make known, I have space to mention only two: 1. A highly important series of facts tending to throw light on the relation between the chemical and the electrical energy of the voltaic pile. For example, that acidulated water, as compared with pure water, increases the chemical action of the pile, but diminishes the electrical; and that those fluids which were found most efficient in exciting the chemical powers of the battery are the most rapidly decomposed when subjected themselves to its action. 2. The indication of the means of obtaining considerable quantities of potassium and sodium by subjecting caustic potash and soda to the contact of iron at a high temperature; and the train of consequences which flowed from the facility of producing those metals. The Memoir which contains the process referred to appeared in the *Moniteur* of the 15th and 16th November 1808. In it was announced the existence of a particular radical, boron, which Davy described a month later in a valuable paper read to the Royal Society of London.

Not the least important, however, of M. Thénard's publications was his *Traité de Chimie*, which has gone through six editions. He had a happy talent for popularizing, without the sacrifice of strict scientific accuracy. His genius lay in arranging the parts, in developing truths in succession, in bringing out the characteristic facts, and causing the whole science to rest symmetrically on them. And the same power of popularizing and arranging was observable in his lectures. The courses which he delivered at the Athenæum, at the Faculty of Medicine, at the *Ecole Polytechnique*, at the College of France, were admirable of their kind. Notwithstanding his intimate acquaintance with the subject, and his long experience as a lecturer, he never presented himself before an audience, without having carefully planned the lecture, and determined the exact order and position which every part should occupy. He used to say that each fact had its own proper place, where alone it could be exhibited in relief, and that it was the duty of the Professor to determine this place beforehand, just as much as it is the duty of an author to clear his sentences of feeble tautology, and to attach the right word to every idea. In consequence of this care, his lecture was always complete, always a continuous lesson on the subject in hand; free alike from deficiency and from exuberance.

It is indeed in his character as a lecturer, that M. Thénard is best studied. On the public platform, the peculiar idiosyncrasies of the whole man came out spontaneously. Let me endeavour to present him to you, as he stands before his class. Imagine a vast amphitheatre capable of holding a thousand persons—every seat occupied—the very lobbies and passages crowded to overflowing. At the back of the contracted space allotted to the Professor and his apparatus, stands a huge black board, well covered with chemical formulæ. The assistant whose duty it has been to prepare the experiments, stands anxiously regarding his work.

The lecturer enters. Your ideas, derived from Hogarth, have perhaps pictured to you a thin spare man with a hatchet face, and you start when your eyes rest on a figure placed in strong relief against the black board, whose firm build and massive countenance more than come up to the typical John Bull of your own land. His broad full eye, set off by a dark mass of hair, first glances at the apparatus, then rises and haughtily scans the audience, as if to measure their capacity, and finally drops on the assistant, who quails beneath its weight. The lecture begins. So clear, so forcible, so continuous, is the stream which flows from the speaker's lips—so appropriate, so neat, and so well performed are the experiments, that the hour passes over quickly and insensibly. But should any accident happen; should the unfortunate assistant have mistaken his directions; woe betide him. The presence of a thousand persons places no restraint on the lecturer's indignation. On one occasion, when he had given way to an unusually violent outburst, an illustrious hearer, said to be Baron Humboldt, thought it his duty to interfere, and request the master to have a little more patience with his assistant. The request was granted, and all went smoothly during the remainder of the lecture. For two days sunshine continued. On the third day, M. Thénard, on entering the room, perceived a portion of the apparatus in a condition which foretold the failure of the experiment. Placing himself right in front of the benevolent stranger, and looking him full in the face, with his finger pointing to the unhappy apparatus, he cried out in the theatrical voice which he inherited from the tragedian Talma, "Friend, I promised to restrain my anger, and I have faithfully kept my word; give me back my promise, or you will see me expire before your eyes." The stranger had no alternative but to bow assent. You may imagine what followed—I will not attempt to describe the scene.

Report says that the assistant was sometimes a match for the professor. On one occasion M. Thénard ironically commiserated him in these words, "Poor fellow, you will never do any good." To which the other replied, "Sir, you compliment me; it is the very same thing Fourcroy predicted of yourself when you were his assistant."

Beneath that rough exterior, and that fiery temper, there lay an honest conscience and a warm heart. Again and again did his assistants tender their resignation, but it was never accepted; and public exhibitions of anger were followed by private acts of kindness. When in 1832, M. Thénard lay ill of a fever, his two assistants, M. Le Canu and M. Clément Desormes, undertook the duty of sitting up alternately by his bedside. One night the latter was so ill of a cough, that the patient forgot his fever in his anxiety to watch over his nurse.

M. Thénard died full of years, and rich in honours and titles.

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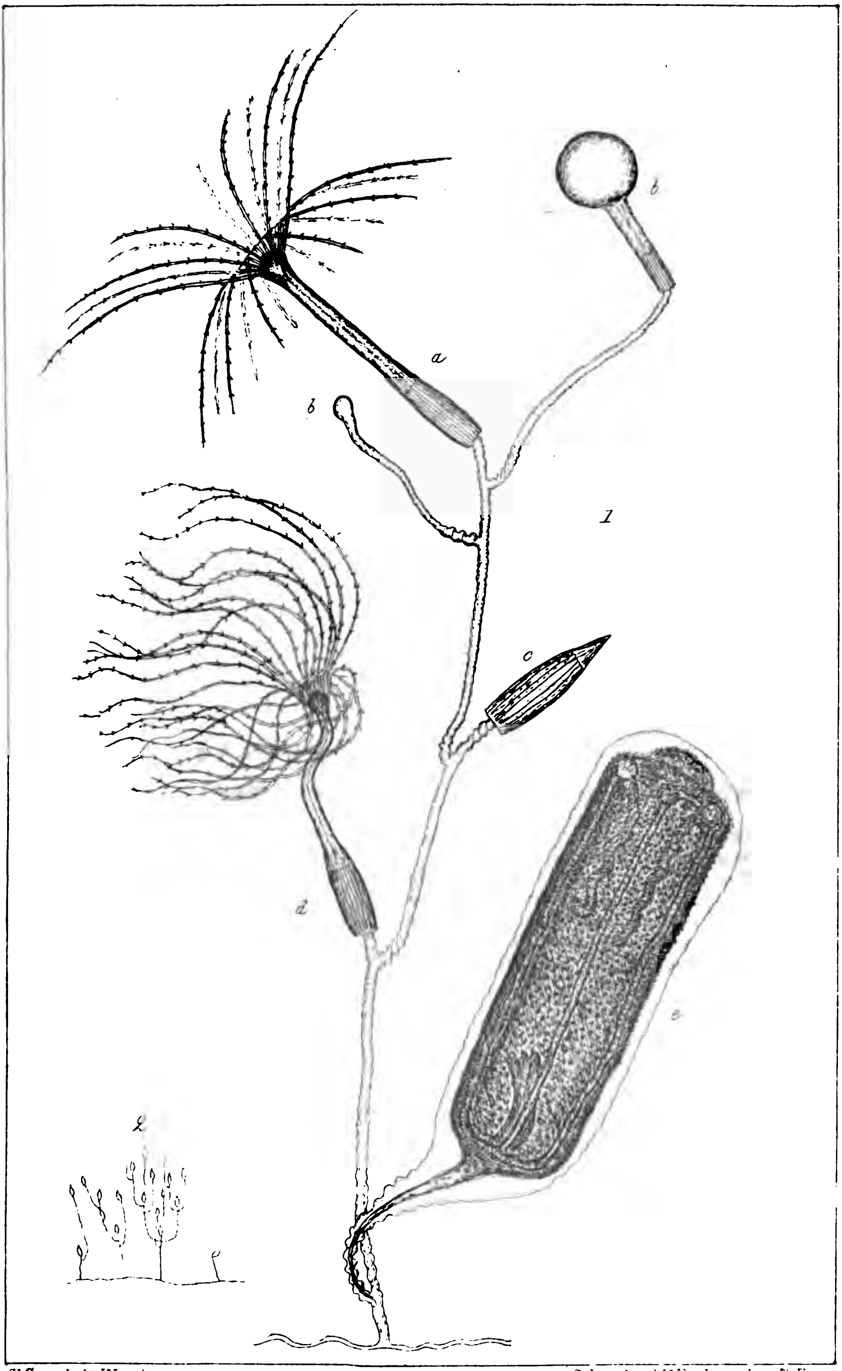

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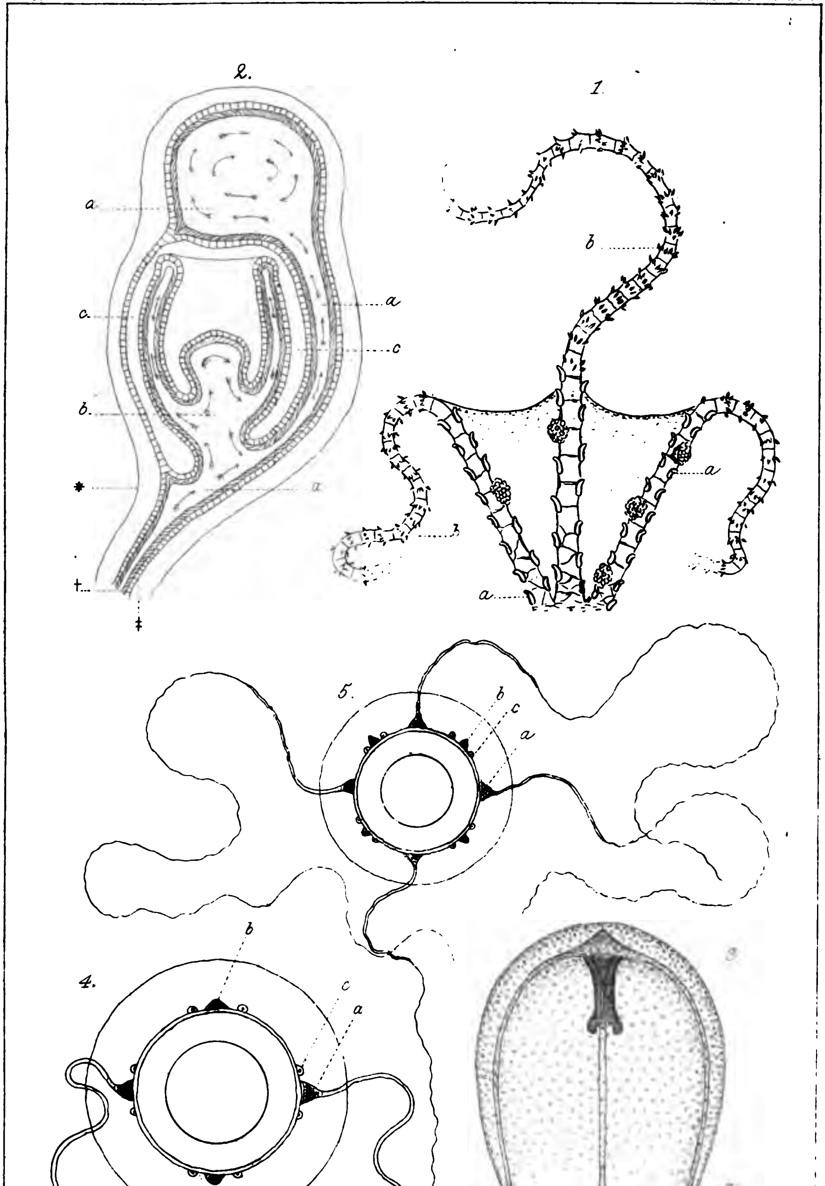

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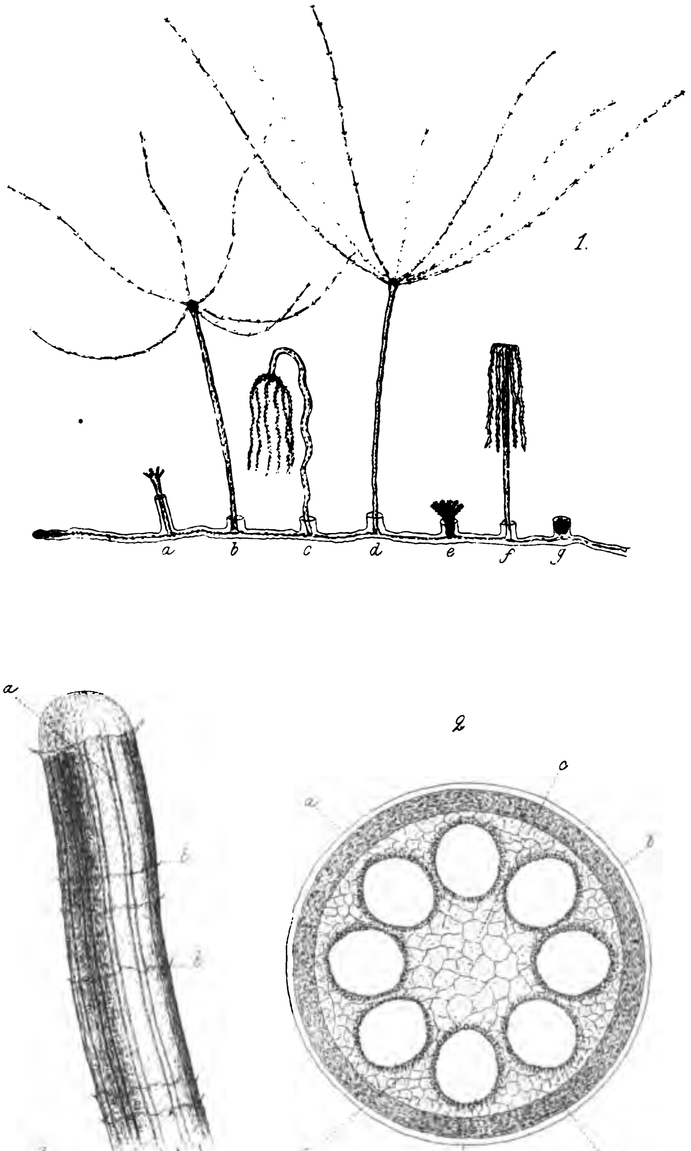

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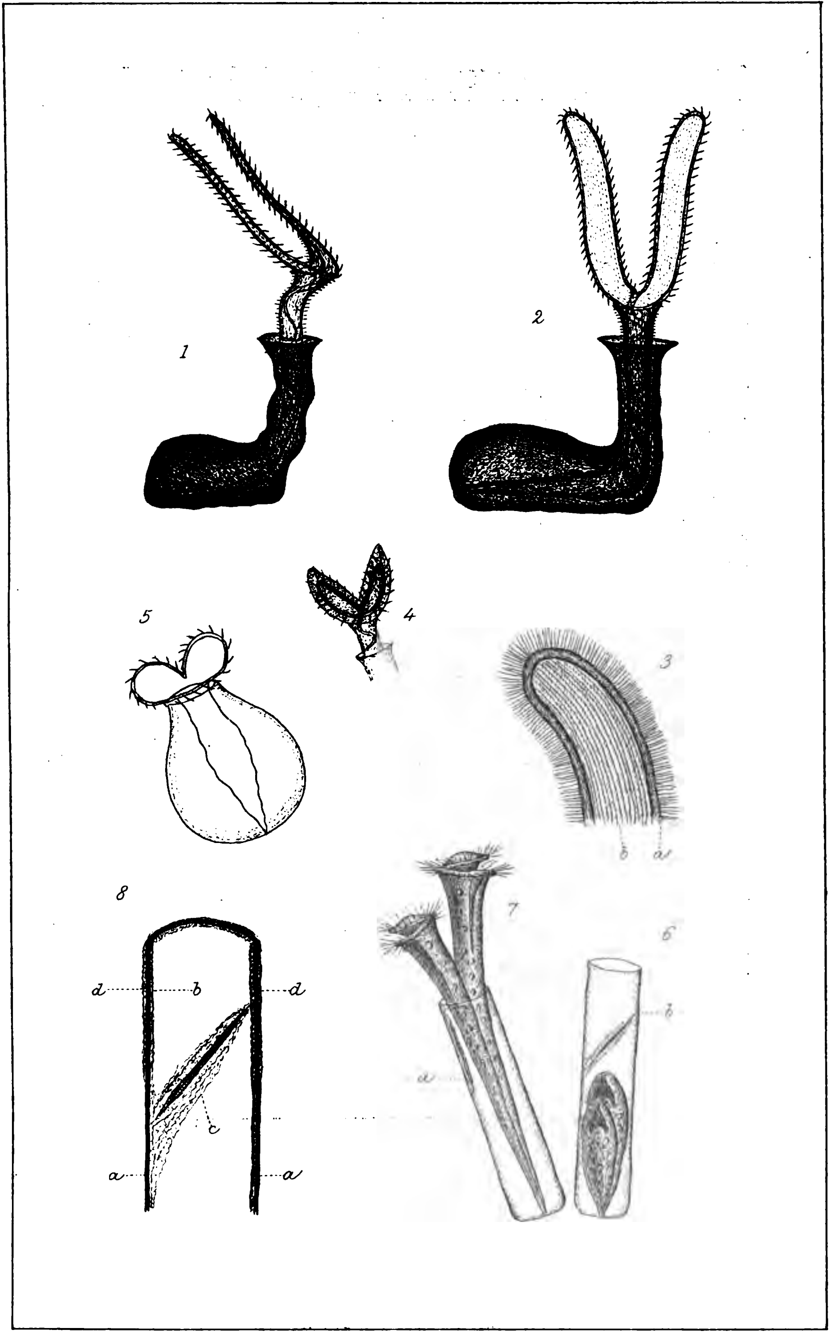

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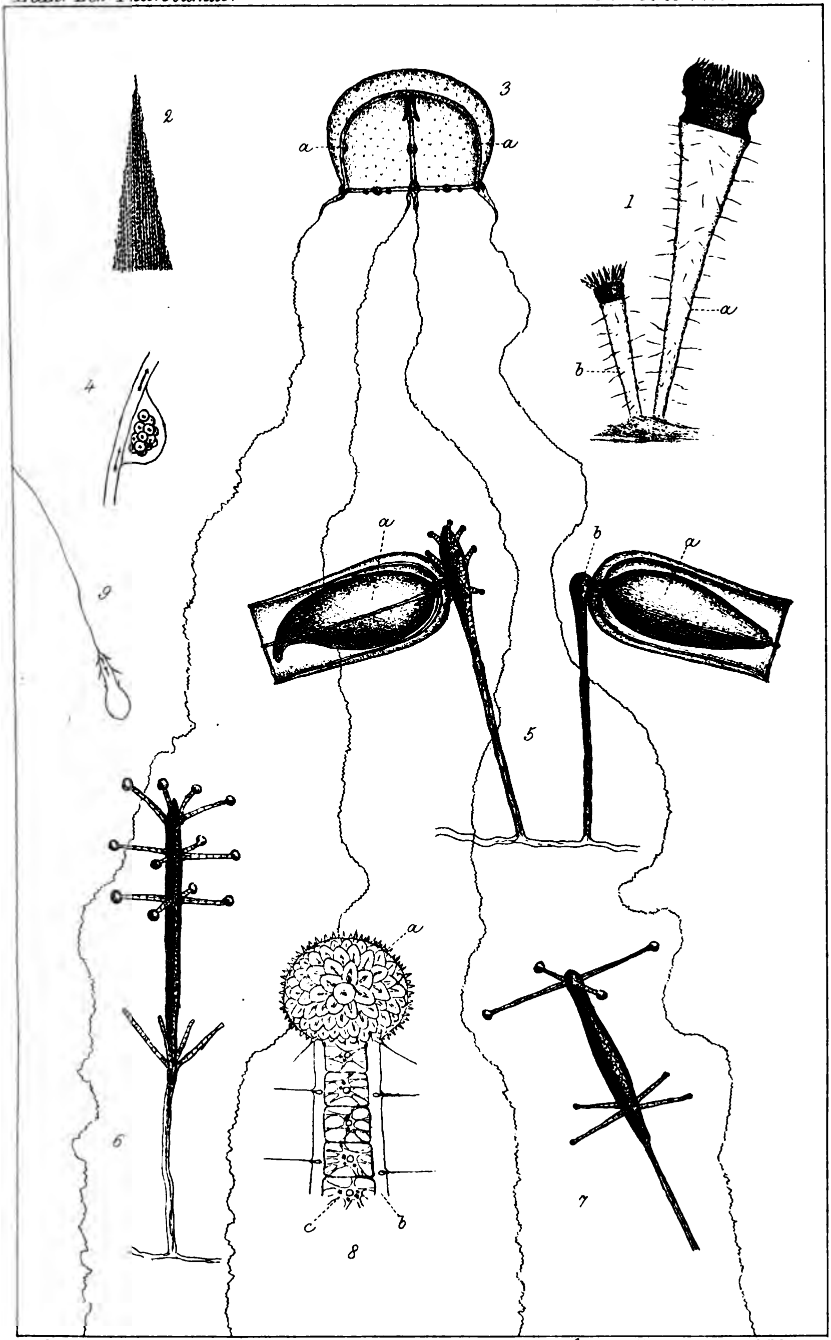

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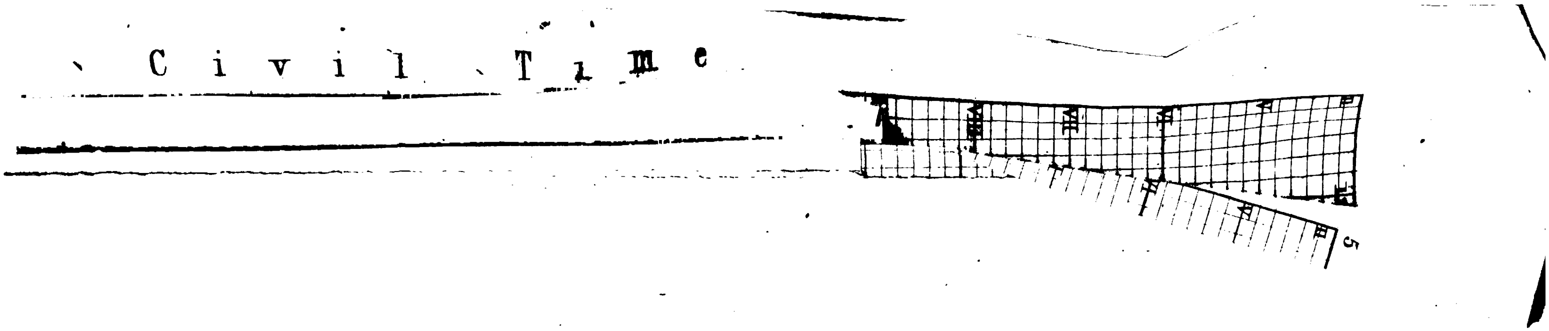
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